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THE THEORY OF ICE MACHINES.

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I.

Chapter I.

§ 1. It has long been known that air is heated or cooled when compressed or dilated.

The mechanical theory of heat defines the conditions under which this heating or cooling is effected, and shows that these effects are proportioned to the external work performed by the air, with the restriction that in expanding the resistance overcome by the gas is always equal to the elastic force of the latter.

If t and t' represent successive temperatures of a unit weight of a permanent gas, which has been compressed or dilated under conditions above stated in producing an amount of work (either resistant or motive) equal to W, we shall have

$$t-t'=\frac{A}{c}W$$

A being the reciprocal of the mechanical equivalent of heat $=\frac{1}{424}$ and c being the specific heat of the gas at constant volume.

In a saturated vapor a part of the thermal equivalent of the external work is transformed into latent heat; the other part alone becomes sensible under the form of external heat.

This is expressed in the fundamental equation

$$c_1(t-t')+(x\rho-x'\rho')=AW$$

in which c_1 is the specific heat of the liquid, x the proportion of vapor in the unit of weight of mixture of liquid and vapor, ρ the latent heat of the vapor and W the external work accomplished.

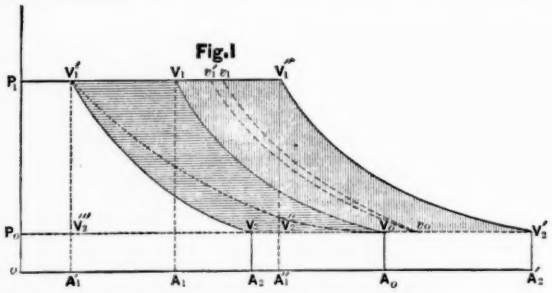
We see from these equations that for the same quantity of heat transformed into work, the range of temperatures must be greater with a gas than with saturated vapors.

§ 2. Whether we employ a permanent gas or a vapor, the apparatus designed for the refrigerating effects is based upon the following series of operations:

Compress the gas or vapor by means of some external force, then relieve it of its heat so as to diminish its volume; next, cause this compressed gas or vapor to expand so as to produce mechanical work and thus lower its temperature. The absorption of heat at this stage by the gas, in resuming its original condition, constitutes the refrigerating effect of the apparatus.

When the cooling takes place at constant pressure, the cycle of operations can be represented by the diagram Fig. 1

Fig. 1



in which the abscissas represent volumes, and the ordinates pressures.

The gaseous body taken at the pressure P_0 and under the volume V_0 is compressed to the tension P_1 and the volume V_1. It is then cooled under constant pressure so that the volume V_1 becomes V_1', then it is allowed to expand, the pressure P_1 becoming P_0 and the volume changing from V_1' to V_2. Finally it is brought to the original volume V_0 by transferring heat to it under constant pressure. The area $V_0V_1V_1'V_2$ represents the work expended and the line V_0V_2 the refrigerating effect obtained.

An inspection of the figure shows that a refrigerating machine is a heat engine reversed.

If instead of cooling the gas, to reduce it from the volume V_1 to V_1', it be heated so as to assume the volume V_1'' greater than V_1 an amount of work is obtained which is represented by the vertically shaded area $V_0V_2'V_1''V_1$; the heat expended is represented by the length V_1V_1''.

It should be noticed that in the case of a permanent gas, the changes from volume V' to V_1' or V_1'' and from V_2 or V_2' to V_0 are accompanied by corresponding changes in temperature. In the case of a condensable vapor these changes are effected at a constant temperature, the addition or subtraction of heat taking effect in an evaporation of the liquid or a condensation of the vapor.

§ 3. From this similarity between heat motors and freezing machines it results that all the equations deduced from the mechanical theory of heat to determine the performance of the first apply equally to the second.

If Q_1 be the quantity of heat taken from or added to a given mass, of compressed gas or vapor, and Q the quantity of heat necessary to subtract from or add to the expanded mass in order to bring it to its initial state, T_0 and T_1 the absolute temperatures corresponding to the volumes V_0 and V_1 and W the work, either active or resistant developed by the machine. The fundamental principle of the mechanical theory of heat, if the gas returns exactly to its primitive condition, affords the equation,

$$Q_1 - Q = AW$$

If the cycle of changes is the so-called cycle of Carnot; that is to say, if the lines V_1V_0, $V_1'V_2$, and $V_1''V_2'$ are adiabatic curves; then we have

$$\frac{Q}{T_0} = \frac{Q_1}{T_1} = \frac{Q_1 - Q}{T_1 - T_0}$$

The quantity of work developed by a heat motor, under these circumstances, is for each heat unit or *calorie*, whatever the intermediate agent,

$$\frac{W}{Q_1} = \frac{I}{A} \cdot \frac{T_1 - T_0}{T_1}$$

The efficiency depends upon the difference between the extremes of temperature.

The performance of a refrigerating machine depends upon the ratio between the calories eliminated and the work expended in cooling.

It is expressed by

$$\frac{Q}{W}$$

and we have

$$\frac{Q}{W} = \frac{AQ}{Q_1 - Q} = A\frac{T_0}{T_1 - T_0}.$$

This result is independent of the nature of the body employed.

Unlike the heat motors, the freezing machines possess the greatest efficiency when the range of temperatures is small, and when the final temperature is elevated.

In a freezing machine employing a vapor, T_0 being the absolute minimum final temperature, this final temperature T_0 in a machine employing a permanent gas is different from the initial temperature T_0, and we have,

$$\frac{T_1}{T_0}=\frac{T_2}{T_2}$$

We can write for the efficiency

$$\frac{Q}{W}=A\frac{T_2}{T_0-T_2}$$

Comparing the efficiencies of the two machines it is evident that the performance becomes less in proportion as we obtain lower final temperatures.

Theoretically there is no advantage in employing a gas rather than a vapor in order to produce cold even if the compression be made without addition or subtraction of heat.

The choice of the intermediate body would be determined by practical considerations based on the physical characteristics of the body, such as the greater or less facility for manipulating it; the extreme pressures required for the best effects, etc.

Air offers the double advantage that it is everywhere obtainable, and that we can vary at will the higher pressures independent of the temperature of the refrigerant. But it is cumbersome, and to produce a given useful effect the apparatus must be of large dimensions.

Liquids on the other hand allow the use of smaller machines, but are obtained only at a greater or less cost.

Furthermore the maximum pressure is determined beforehand by the temperature of the refrigerant, and depending on the nature of the volatile liquid; this pressure is often very high.

§ 4. The foregoing conclusions are based on the hypothesis that the compression and expansion follow the adiabatic lines V_0V_1 and $V_1'V_2$, that is to say that the changes of volume and pressure follow the cycle of Carnot.

This hypothesis is realized when the cooling is accomplished outside of the compression cylinder and after the gas has been raised to the pressure P_1.

If the compression be effected according to some cycle different from Carnot's, the efficiency, if it be a heat motor, would be diminished, but in a freezing machine it would be greater or less, depending upon the manner in which the successive operations were effected.

Suppose for example that instead of cooling, the gaseous body outside the compression cylinder, it be done during compression within the cylinder in such a manner as to maintain a constant temperature. This hypothesis would be graphically represented in Fig. 1 by replacing the adiabatic curve V_0V_1 by the isothermic curve V_0V_1'. The work of resistance of the machine would then be represented by the curvilinear triangle $V_0V_1V_1'V_2$. The quantity of negative heat produced represented by the line V_0V_2 remains the same. The efficiency of the freezing machine would be thus augmented as the resistant work of the motor would be less than the preceding case for the same quantity of negative heat produced.

The cooling of vapors during compression is not readily realized, since it is effected at a constant temperature and one which is lower than the refrigerant. It is realized though somewhat incompletely in the case of permanent gases since their temperature during compression is above that of the refrigerant.

§ 5. The efficiency is calculated in the following manner.

We suppose the compression to be made at a constant temperature. Then by Marriotte's Law we have $P_1V_1=P_0V_0$.

The work of resistance to compression would be

$$W_r = P_0V_0 l\frac{V_0}{V_1}=RT_0 l\frac{V_0}{V_1}$$

and we shall have as in the preceding case.

$$AW_r = Q_1$$

R is a constant, uniform for the air at 29.27 inches and a unit of weight is supposed taken.

The gas dilating from the temperature T_0 to T_2 without gaining or losing heat, we shall have for the work of dilatation, inclusive of the work at full pressure during introduction;

$$AW_m = kc(T_0 - T_2) = Q.$$

The performance is represented by

$$A\frac{Q}{Q_1 - Q}$$

and we have

$$\frac{Q}{W_r - W_m} = A\frac{Q}{Q_1 - Q} = \frac{kc(T_0 - T_2)}{RT_0 l\frac{P_1}{P_0} - \frac{kc}{A}(T_0 - T_2)}.$$

We have also

$$\frac{c}{A} = \frac{R}{k-1};$$

k is the ratio of specific heat at constant pressure to the specific heat at constant volume; this ratio is $=1.41$ and is the same for all permanent gases.

It follows then

$$A\frac{Q}{Q_1 - Q} = A\frac{T_0 - T_2}{\left(\frac{k-1}{k}\right)T_0 l\frac{P_1}{P_0} - (T_0 - T_2)}$$

If the compression follows an adiabatic curve, we shall have for the efficiency—calling T_1 the absolute final temperature of the compression

$$A\frac{Q}{Q_1 - Q} = A\frac{T_0 - T_2}{T_1 - T_0 - (T_0 - T_2)}$$

and

$$\frac{T_1}{T_0} = \left(\frac{P_1}{P_0}\right)^{\frac{k-1}{k}}$$

It is easy to show that

$$T_1 - T_0 \text{ or } T_0\left\{\left(\frac{P_1}{P_0}\right)^{\frac{k-1}{k}} - 1\right\}$$

is greater than

$$\frac{k-1}{k}T_0 l\frac{P_1}{P_0}$$

and consequently that the efficiency in the first case is less than in the second.

The employment of air presents a certain theoretical advantage over volatile liquids, inasmuch as it admits of cooling to a certain extent during compression.

We will now examine in succession some of the recently invented freezing machines (*machines a froid*). The Air Machine of M. Giffard; the Sulphurous Acid Machine of M. Pictet, and the Ammonia Machine of M. Carré.

Chapter II.

Giffard's Air Machine.

§ 7. This machine consists of a single-acting cylinder A, the piston of which is furnished with two valves opening from without inward. This cylinder is surrounded with a jacket leaving a space within which circulates a current of cold water.

There is a second cylinder, B, also single-acting, and having a solid piston, and with a diameter a little smaller than the first. At the bottom of this cylinder are two openings closed by valves, opening, one outward and the other inward, and operated by levers which are worked by cams on the driving shaft.

The pistons are driven by crank connections with the main shaft.

The condenser R is a surface condenser and receives a current of cold water from the envelope of the compressor cylinder A. A Reservoir of wrought iron, R′, is connected with the condenser by a tube and communicates also with the bottom of the expansion cylinder B.

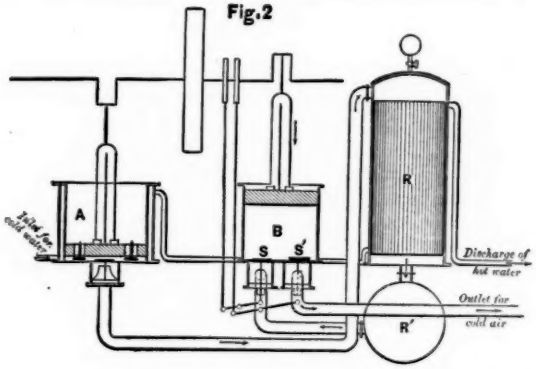
§ 8. The air taken in at ordinary pressure is compressed in the cylinder A till it has the density of that in the reservoir; it is then allowed to flow into the condenser R and the reservoir R′. During this passage it loses a great part of the sensible heat which it attains during compression, and is brought nearly to the temperature of the surrounding air.

During this time the valve s of the cylinder B opens and permits a certain amount of air equal in weight to that which is expelled from A, to pass from the reservoir into the cylinder producing a certain amount of work. Then the valve s closes,—the air in the cylinder B expands producing again work which may be deducted from the work of compression and the temperature is lowered. When the piston B reaches the upper limit of its stroke, the valve s' opens and the cooled air as the piston descends escapes by the tube T.

The cooling experienced by the air, during compression, by contact with the cooled sides of the cylinder is scarcely sensible.

The machine therefore acts under conditions set forth in § 2 and we know that its useful effect cannot exceed the value

Fig. 2



$$A\frac{T_0}{T_1-T_0} \quad \text{or} \quad A\frac{T_2}{T_0-T_2}$$

By means of the adjustable cams we can regulate at will the action of the valves s and s'. If we shorten the time of admission into the cylinder B, the pressure will increase in the reservoir; for the amount flowing into B should be equal to that forced into the reservoir from A. The temperature of the air expelled will then be less. If, on the contrary, we increase the time of admission the reservoir pressure will diminish, and the temperature of outflowing air will be increased.

The apparatus presents then this important peculiarity—that we can vary the useful effect of the machine at will, through wide limits.

As the air leaves B, at the pressure of the atmosphere, the minimum limit of pressure is established, below which the expansion cannot be pushed, and which is controlled by the relative dimensions of the two cylinders.

We will proceed to calculate the cooling effect produced by this machine and the corresponding work required. We shall neglect at first the effect of waste spaces in the machine, and of watery vapor in the air.

§ 9. Let P_0, t_0 and T_0 be the pressure and temperature (counted from absolute zero) of the air.

V_0 the volume described by the piston A.

V_1 the volume of air when at pressure P_1.

V_1 is then the volume described by the piston during the outflow.

m = weight of air whose volume passes from V_0 to V_1.

P_1, t_1 and T_1 the pressure and temperature of compressed air delivered from A.

V_1' t_1' and T_1' the volume and temperature after passing into the condenser.

V_2 the total volume described by piston B.

P_2, t_2 and T_2 the pressure and temperature of the air at the end of the course of this piston.

During compression the cooling by simple contact with the sides of the cylinder is insignificant. We shall neglect this and also assume that no heat is received from the sides of the cylinder B.

FIRST PERIOD: COMPRESSION.

§ 10. When air is compressed without losing or gaining heat, the pressure and temperature at each instant bear the relation to each other expressed by the equation

$$P_0V_0^k = P_1V_1^k \qquad (1)$$

in which k is the ratio of specific heat of constant pressure to the specific heat of constant volume.

$$k=\frac{0.23751}{0.16844}=1.41$$

Gay Lussac's law affords,

$$P_0V_0=RmT_0 \qquad (2)$$

and $$P_1V_1=RmT_1 \quad (3)$$

From equations 1 2 and 3 we deduce

$$\frac{T_1}{T_0}=\left(\frac{P_1}{P_0}\right)^{\frac{k-1}{k}} \quad (4)$$

$$\frac{T_1}{T_0}=\left(\frac{V_0}{V_1}\right)^{k-1}. \quad (5)$$

The work of the resistance to compression and outflow is

$$W_r=\frac{k}{k-1}(P_1V_1-P_0V_0). \quad (6)$$

We have elsewhere

$$\frac{k}{k-1}=\frac{kc}{AR}.$$

c being the specific heat of air of constant volume.

Equation (6) then becomes

$$W_r=\frac{mkc}{A}(T_1-T_0). \quad (7)$$

SECOND PERIOD: COOLING.

The air is cooled in the condenser under constant pressure. The volume changes from V_1 to V_1', and the temperature from t_1 to t_1'.

we have; $$V_1'=V\frac{T_1'}{T} \quad (8)$$

and the quantity of heat imparted to the water of the condenser is;

$$Q_1=mkc(T_1-T_1') \quad (9)$$

If $T_1'=T_0$ then $R_1=AW_r$

THIRD PERIOD; EXPANSION.

The volume V_1' of air enters the cylinder B yielding an amount of work equal to P_1V_1'. It expands from V_1' to V_2 without gain or loss of heat. We have then:

$$P_1V_1'^k=P_2V_2^k, \quad (10)$$

$$P_1V_1'=RmT_1', \quad (11)$$

$$P_2V_2=RmT_2 \quad (12)$$

whence $$T_2=T_1'\left(\frac{P_2}{P_1}\right)^{\frac{k-1}{k}}. \quad (13)$$

The work performed by the air is

$$W_m=\frac{k}{k-1}(P_1V_1'-P_2V_2) \quad (14)$$

or $$W_m=\frac{mkc}{A}(T_1'-T_2) \quad (15)$$

The resistances to be overcome by external force amount to

$$W_r-W_m=\frac{mkc}{A}[(T_1-T_1')-(T_0-T_2)]. \quad (16)$$

If the machine works properly, the final pressure P_2 should be equal to the atmospheric pressure.

The equations (10) (12) and (13) give

$$\left.\begin{aligned}\frac{V_2}{V_1'}&=\frac{V_0}{V_1}\\ \frac{V_2}{V_0}&=\frac{T_1'}{T_1}\end{aligned}\right\} \quad (17)$$

or

and $$\frac{T_2}{T_1'}=\frac{T_0}{T_1} \quad (18)$$

Equation (17) expresses the ratio which should exist between the volumes of the two cylinders, in order that the air be finally expelled at atmospheric pressure, after having been compressed by a force P_1.

The negative heat (cooling), produced by the apparatus, is the quantity of heat necessary to restore the air from the temperature t_2 to the temperature t_0, under constant pressure.

$$\left.\begin{aligned}Q&=mkc(T_0-T_2)\\ Q&=mkcT_0\left(1-\frac{T_1'}{T_1}\right)\end{aligned}\right\} \quad (19)$$

or

§ 11. Since a given weight of air is restored, at the end of the operation, to the same temperature and pressure it had at the beginning it follows, that it has been through a perfect cycle and we have from the mechanical theory of heat;

$$Q_1=A(W_r-W_m)+Q$$

The theoretical performance of the machine is, calling it u,

$$u=\frac{Q}{W_r-W_m}=A.\frac{T_0-T_2}{(T_1-T'_1)-(T_0-T_2)},$$

$$u=A.\frac{1}{\frac{T_1-T'_1}{T_0-T_2}-1}$$

and as we have from equation (18)

$$\frac{T_1-T'_1}{T_0-T_2}=\frac{T_1}{T_0}=\frac{T'_1}{T_2}$$

we get finally

$$u=A.\frac{T_0}{T_1-T_0}=A.\frac{T_2}{T'_1-T_2}, \quad (20)$$

a result already found in § 3 by suppos-

ing $T_1' = T_0$. If $T_1 > T_0$ the useful effect is diminished.

The efficiency of the machine will be all the greater as T_1 approaches in value to T_0; that is to say as it is urged at a lower pressure into the reservoir. But as we lower the pressure of working, the quantity of negative heat produced diminishes also and becomes nothing when $T_1' = T_1$.

The necessary driving power $W_r - W_m$ which we proceed to calculate, should be augmented by the passive resistances.

If we consider the refrigerating machine as composed of two distinct machines driven by the same shaft, we are led to consider that the work of the passive resistances is proportional not to the final work $W_r - W_m$ but rather to the sum of the work developed in the two cylinders $W_r + W_m$. Considering the simplicity of the machine, the small amount of friction, and the absence of a stuffing box, we can admit that the work of the passive resistances should not exceed eight per cent of the above total work.

The resistance of the machine is then $1.08W_r - 0.92W_m$.

The following table gives the amount of refrigeration obtained, and the work expended, by passing a cubic meter of dry air through the machine; the pressures in the reservoir varying from $1\frac{1}{2}$ to $4\frac{1}{2}$ atmospheres. The temperature of the external air is taken at 15°; the temperature of the air leaving the condenser at 18°; temperature of the water about 13° $V_0 = 1$, $T_0 = 288$ and $m = 1^k.266$.

P_1 (Atmospheres.)	Temperature of air after compression. t_1.	Temperature of outflowing air. t_2.	Diminution obtained. $t_0 - t_2$.	Negative calories obtained.	Work expended, theoretical kilogrammeters.	Work expended, effective.	No. of negative calories developed.			
							Per kilogrammeter, theoretic.	Per horse power per hour, theoretical.	Kilogrammeter really expended.	Per horse power per hour.
	degrees	degrees	degrees	cal.			cal.	cal.	cal.	
$1\frac{1}{2}$	51,04	—14,36	29,36	8,548	454,26	1.070,5	0,01882	5.081, 4	0,007985	2.156
2	79,31	—35,12	50,12	14,591	1.381,26	2.481,6	0,01056	2.851, 2	0,00588	1.587
$2\frac{1}{2}$	102,93	—49,55	64,55	18,792	2.515,64	3.991,1	0,00747	2.016, 9	0,00471	1.281
3	123,39	—62,58	77,58	22,586	3.556,3	5.412,3	0,00635	1.714, 8	0,00417	1.126
$3\frac{1}{2}$	141,57	—70,84	85,84	24,991	4.657,3	6.724,2	0,00537	1.448, 8	0,00372	1.003
4	157,98	—78,54	93,54	27,232	5.732,5	8.097,0	0,00475	1.282, 6	0,00336	907
$4\frac{1}{2}$	173,00	—85,09	100,09	29,375	6.778,0	9.297,0	0,00433	1.170, 2	0,00316	853

§ 12. An examination of the table shows the enormous influence that the passive resistances exert upon the efficiency of air machines. It is one of the consequences of the inherent cumbrousness which follows from the use of this body in a thermic machine.

The useful effect produced is not increased in proportion to the increase of pressure. It is of no advantage to employ pressures higher than about $4\frac{1}{2}$ atmospheres. Aside from the diminution of efficiency of the air at high pressures, a loss is occasioned by heat developed in the compressor, and which extends to other working parts of the machine. We have said above that, with a given machine we can vary at will the pressure P_1 by varying the length of time of the opening of the admission valve in the cylinder B. If the time be shortened the pressure and the cooling effect are both increased; and if the time be increased P_1 is diminished. It is necessary that we should vary at the same time the working of the emission valve, so that it opens at the moment when the piston shall have passed through a space equal to $V_0 \frac{T_1'}{T_1}$ corresponding to the atmospheric pressure on the inside of the expansion cylinder.

A machine whose dimensions and velocity are such that it uses 1000 cubic meters of air per hour will produce from 8.548 to 29.375 negative calories and upwards

per hour, provided that the driving power varies from 4 to 34 horse power.

Practically however the efficiency of air machines is not so great as is indicated by the above table as no account has yet been taken of watery vapor in the air, nor of lost spaces in the machine.

We proceed to examine the influence of these two causes of loss.

INFLUENCE OF MOISTURE IN THE AIR.

§ 13. This influence is not to be neglected. The vapor contained in the air condenser on the sides of the expansion cylinder, and parts with its latent heat of vaporization so that the final temperature of the air is higher than it would have been if dry.

Furthermore the snow produced from this moisture accumulates around the orifice of cold air outlet and we cannot readily utilize the cold which is required to produce it. For these two reasons, but especially for the latter, the moisture of the air causes a notable loss.

We proceed to calculate the volume and the temperature of the air at the end of the expansion under the supposition of a known hygrometric state of the atmosphere, from which we can easily deduce by the tables the pressure of the vapor p_0 and its weight μ_1.

In the compression cylinder of watery vapour not being near the saturation point, and exerting a feeble pressure will behave nearly as a perfect gas; its volume and its temperature are represented by the relations pv^k=a constant, in which k=1.41 and pv=R′mT;

$$R' = \frac{R}{0.622} = 47.061.$$

The total pressure of air and vapor being represented by P, the pressure of the vapor being p, that of the air alone will be P−p and we shall have preserving our former notation:

$$P_1 V_1^k = P_0 V_0^k, \quad (21)$$

$$p_1 V_1^k = p_0 V_0^k, \quad (22)$$

$$(P_0 - p_0) V_0 = R m T_0, \quad (23)$$

$$p_0 V_0 = R' \mu_1 T_0, \quad (24)$$

$$(P_1 - p_1) V_1 = R m T_1, \quad (25)$$

$$p_1 V_1 = R' \mu_1 T_1. \quad (26)$$

The work of the resistance to compression is

$$\left.\begin{aligned} W_r &= \frac{k}{k-1}(P_1 V_1 - P_0 V_0) \\ \text{or}\quad W_r &= \frac{A}{k}(mc + \mu_1 c')(T_1 - T_0) \end{aligned}\right\} \quad (27)$$

c' is the specific heat under constant volume of the superheated vapor

$$c' = 0{,}3407.$$

After cooling the volume becomes

$$V'_1 = V_1 \frac{T'_1}{T_1} \quad (28)$$

and we have

$$p_1 V'_1 = R' \mu_1 T'_1.$$

From equations 21 and 22 we can deduce the pressure in the reservoir.

We can determine by examining a table of tensions of saturated steam whether the pressure p_1 is greater or less than the pressure which corresponds to the temperature T_1'. If it be less the air will not be saturated with vapor when leaving the condenser, and the heat absorbed by the latter will be:

$$Q_1 = k(mc + \mu_1 c')(T_1 - T_1')$$

If the pressure p_1 is greater than the pressure p_1', corresponding to the temperature T_1' for saturated steam, there will be a condensation of some of the vapor in the condenser; the amount condensed will be

$$\mu_1(1 - x_1')$$

and the pressure of the vapor entering into the cylinder B will be p_1', that of the air being $P_1 - p_1'$.

We shall have also:

$$x_1' = \frac{p_1'}{p_1} = \frac{p_1'}{p_0} \cdot \frac{P_0}{P_1}$$

We see that the quantity of vapor not condensed by the cooling, and passing into the expansion cylinder, will continually diminish in proportion as the working pressure is raised. The influence of the humidity in the air will therefore be less as the pressure is made greater.

The weight of the mixture of air and vapor, which is $m + \mu_1$ if there is no condensation in the cooler or $m + \mu_1 x_1'$ if there is a condensation, is carried into the cylinder B where it encounters the surfaces cooled during the preceding stroke. We can neglect the influence of these cold surfaces upon the air alone, but not upon the mixture of air and

vapor. The latter is converted into frost which releases a certain amount of heat to be imparted to the metal, and which during the expansion is restored to the air.

Suppose at first that there is no condensation in the cooler, there is conveyed to the cylinder a weight μ_1 of saturated, or nearly saturated, vapor at the temperature T_1'. We may assume, considering the very low temperature of the surfaces, that all the vapor is condensed here; it will disengage a quantity of heat C, which is approximately equal to $\mu_1(r_1'+79)$. r_1' being the latent heat of the vapor corresponding to the temperature t_1', 79 is the latent heat of water released on freezing.

The heat C is gradually restored to the air during expansion.

The pressure of the air becomes P_1, and the volume introduced into the cylinder is

$$V_1'' = \frac{Rm T_1'}{P_1}.$$

The differential equation of the work is

$$\frac{c}{A} m dT + \frac{c_1}{A} \mu_1 dT - \frac{dC}{A} = -P dV$$

$$= -Rm T \frac{dV}{V};$$

c_1 being the specific heat of ice, $=0{,}5$

$$\text{or } \left(\frac{c}{AR} m + \frac{c_1}{AR} \mu_1\right) \frac{dT}{T} - \frac{dC}{ART} = -m \frac{dV}{V}.$$

We do not know the law of relation between C and T_1, that is, how to communicate to the air the heat released from the water and ice formed. We are forced to make a hypothesis which is not rigorously exact, but which is sufficiently approximate.

We will suppose that the transmission is proportioned to the fall of temperature, and therefore that

$$dC = -\mu\gamma dT$$

in which

$$\gamma = \frac{r_1' + 79}{T_1' - T_2}$$

whence we have;

$$\frac{1}{AR} (mc + \mu_1 c_1 + \mu_1 \gamma) \frac{dT}{T} = -m \frac{dV}{V}$$

integrating we get

$$\frac{c}{AR}\left(1 + \frac{\mu_1 c_1 + \mu_1 \gamma}{mc}\right) l \frac{T_1'}{T_2} = l \frac{V_2}{V_1''}$$

$$\text{or } \frac{1}{k-1}\left(1 + \frac{\mu_1 c_1 + \mu_1 \gamma}{mc}\right) l \frac{T_1'}{T_2} = l \frac{V_2}{V_1''} \quad (29)$$

we have furthermore

$$P_0 V_2 = Rm T_2,$$

$$P_1 V_1'' = Rm T_1',$$

whence

$$\frac{T_1'}{T_2} = \frac{P_1 V_1''}{P_0 V_2}.$$

Equation 29 can then be written;

$$\frac{1}{k-1}\left(k + \frac{\mu_1 c_1 + \mu_1 \gamma}{mc}\right) l \frac{T_1'}{T_2} = l \frac{P_1}{P_0}. \quad (30)$$

We can obtain the value of T_2 by successive approximations.

An approximate value for T_2 is found to be

$$T_2 = \frac{\left\{mkc + \mu_1 c_1 - \frac{(k-1)mc}{2} \frac{P_1}{l P_0}\right\} T_1' + \mu_1 (r_1' + 79)}{mkc + \mu_1 c_1 + \frac{(k-1)mc}{2} l \frac{P_1}{P_0}}$$

Suppose now that condensation occurs in the cooler, we find by the tables the pressure of p_1' of saturated vapor of temperature T_1' and we can deduce the weight of the vapor condensed in the cooler.

We shall have then;

$$C = \mu_1 x_1' (r' + 79)$$

and

$$\gamma = x_1' \frac{r_1' + 79}{T_1' + T_2}$$

The equations 29 and 30 apply in this case as in the preceding.

The quantity of disposable negative heat is;

$$Q = mkc(T_0 - T_2) \quad (31)$$

since we suppose the negative heat of the snow formed to be lost.

Finally the work produced by the expansion is;

$$W_m = P_1 V_1'' + \frac{mc}{A}\left(1 + \frac{\mu_1 c_1 + \mu_1 \gamma}{mc}\right) (T_1' - T_2) - P_0 V_2 \quad (32)$$

$$\text{or } W_m = \frac{mkc + \mu_1 (c_1 + \gamma)}{A} (T_1' - T_2) \quad (33)$$

If there is a condensation in the cooler, we should replace μ_1 in equations 32 and 33 by $\mu_1 x_1'$.

§ 14. The following table gives the cooling and general effect obtained from a cubic meter of air supposing a hygro-

Pressure atmospheres.	Temperature in compressor.	Temperature of expelled air.	Cooling obtained. t_0-t_1.	Negative calories.	Theoretic work, kilometers.	Effective work expended.	No. of negative calories obtained.				Weight of vapor carried into expansion cylinder.
							Per kilogrammeter, theoretic.	Per theoretic horse power per hour.	Per effective kilogrammeter.	Per effective horse power.	
	degrees	degrees	degrees	cal.			cal.	cal.	cal.	cal	
1½	51,04	— 0,43	15,43	4,455	291	981	0,01531	4.134	0,00454	1.226	0,00626
2	79,31	—21,70	36,70	10,600	1.257	2.429	0,00843	2.276	0,00436	1.177	0,00626
2½	102,93	—37,25	52,25	15,090	2.286	3.843	0,00660	1.785	0,00393	1.061	0,00615
3	123,39	—50,80	65,80	18,997	3.486	5.354	0,00545	1.472	0,00355	959	0,00504
3½	141,57	—61,53	76,53	22,095	4.592	6.730	0,00481	1.299	0,00328	886	0,00439
4	157,98	—70,58	85,58	24,710	5.674	8.050	0,00435	1.176	0,00307	829	0,00384
4½	173,00	—78,26	93,26	26.928	6.714	9.304	0,00409	1.104	0,00290	783	0,00342

metric state of ½ and a temperature of 15°. The weight of the air is then $1.^k 2157$ instead of $1.^k 226$ which is the weight of dry air at this temperature.

We have also $p_0 = 85.^k 8$ and $\mu_1 = 0.^k 00626$.

In comparing this table with the table of § 11 we see that the influence of the humidity of the air upon the results obtained is the greater when the pressure is low. We have made a similar remark in reference to the passive resistances. The theoretical advantage therefore of low pressures is practically much diminished by these causes of loss.

It is possible to neutralize almost completely the influence of moisture in the air. To accomplish this it would suffice to employ the air after it had produced its cooling effect and had parted with its moisture. It would be necessary to make the refrigerating machine a closed machine, making the same quantity of air serve indefinitely. The cooling would be produced by causing the cooled air to pass through an apparatus surrounded by some liquid not easily frozen, such as a solution of calcium or magnesium chloride. A part of the negative calories would thus be used, as well as by direct contact, and so many as are not used would not be lost, as the air passes directly to the compressor A, not at 15° as before, but $a-8°$ or $-10°$ of temperature. We think that it is only in this way that we can improve the air machine so that it can compare favorably with the machines using a liquefrable gas.

INFLUENCE OF WASTE SPACES.

§ 15. We will suppose the air to be dry in order to avoid complexity in our calculations.

Preserving our previous notation and calling v the amount of useless space in the compression cylinder, and v' that of the expansion cylinder; μ the weight of air enclosed in the space v at the end of the compression, we have;

$$P_0(V_0+v)^k = P_1(V_1+v)^k \quad (34)$$

$$P_0(V_0+v) = R(m+\mu)T_0 \quad (35)$$

$$P_1V_1 = RmT_1 \quad (36)$$

$$Pv_1 = R\mu T_1 \quad (37)$$

m being the weight of dry air driven out of the compressor.

Equations (34), (35), (36) and (37) give by elimination of μ

$$T_1 = T_0\left(\frac{V_0+v}{V_1+v}\right)^{k-1} \quad (38)$$

and

$$T_1 = T_0\left(\frac{P_1}{P_0}\right)^{\frac{k-1}{k}} \quad (39)$$

The work of resistance to compression, taking account of the work restored to the piston as it begins to ascend, by the air in the waste space, expanding from P_1 to P_0, is;

$$W_r = \frac{k}{k-1}(P_1V_1 - P_0V_0) + \frac{1}{k-1}P_0v\frac{V_0-V_1}{V_1+v}; \quad (40)$$

For the cooling period;

$$V_1' = V_1\frac{T'}{T_1} \quad (41)$$

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and $$P_1V'_1=RmT_1' \qquad (42)$$

The heat Q_1 absorbed by the water of the condenser is;

$$Q_1=mkc(T_1-T_1') \qquad (43)$$

PERIOD OF EXPANSION.—The air coming from the reservoir R′ under pressure P_1 and the temperature P_1', should at the moment of opening of the inlet valve cause the air in the waste space and whose volume is v', to change its pressure from P_0 to P_1. This influences the temperature T_1'' of the mixture, also the weight m' of the air which passes from the reservoir into the waste space.

The dimensions of the reservoir being very large in comparison to the waste spaces, we may assume that no change occurs either in temperature or pressure of the reservoir, while the waste spaces are filled with air at the pressure P_1.

Calling μ' the weight of the air enclosed in the waste space at the moment that the inlet valve opens. We have;

$$P_0v'=R\mu'T_2; \qquad (44)$$

T_2 being the final temperature of the expanded air.

The stored up work of this air is;

$$\frac{c}{A}\mu'T_2.$$

The weight m' of air filling the waste space, and having a temperature T_1' and a pressure P_1 has a stored energy of $\frac{c}{A}m'T_1'$.

After the waste space is filled, the stored up energy of the total quantity of air $m'+\mu'$ contained there is

$$\frac{c}{A}(m'+\mu')T_1''$$

and we have furthermore;

$$P_1v'=R(m'+\mu')T_1''.$$

As we suppose there is neither loss nor gain of heat from the exterior, the difference between the stored energy of the mixture after the mass m' is introduced, and the sum of the stored energies of the masses m' and μ' before mixing is equal to the external work performed.

This exterior work is evidently P_1v_1', and calling the volume of m' before its introduction into the cylinder under pressure P_1 and temperature T_1' equal to v_1', then;

$$P_1v_1'=RmT_1'.$$

We have also

$$-\frac{c}{A}\mu T_2-\frac{c}{A}m'T'_1+\frac{c}{A}(m'+\mu')T_1''=RmT_1',$$

Replacing $\frac{c}{A}$ by $\frac{R}{k-1}$ and combining with equations 44 and 45

$$m'=\frac{(P_1-P_0)v'}{kRT'} \qquad (46)$$

$$\left.\begin{aligned} &\text{and}\quad T_1''=\frac{km'T_1'+\mu'T_2}{m'+\mu'}\\ &\text{or}\quad T_1''=\frac{kP_1T_1'T_2}{P_1T_2+P_0(kT_1'-T_2)}\end{aligned}\right\} \qquad (47)$$

When the inlet valve closes, the piston has described a volume V_1'', which has been filled by the weight m'' of air at pressure P_1 and temperature T_1'. We have then;

$$m'+m''=m$$

There is no external work performed upon the total mass of air, since the negative work of the piston P_1V_1'' is exactly equal to the positive work exerted by the air of the reservoir. The weights and temperatures of the air at the beginning and the end of the introduction possess the following relations:

$$\frac{c}{A}(m'+\mu)T_1''+\frac{c}{A}m''T_1'=\frac{c}{A}(m'+m''+\mu')T_1'''.$$

T_1''' being the temperature at the end of the introduction.

This equation gives;

$$\left.\begin{aligned} &T_1'''=\frac{(m'+\mu')T_1''+m''T_1'}{m+\mu'}\\ &\text{or}\quad T_1'''=\frac{P_1(V_1'+v')-\frac{1}{k}(P_1-P_0)v'}{P_1V'_1T_2+P_0v'T_1'}\,T_1'T_2\end{aligned}\right\} \qquad (48)$$

we also have

$$P_1(V_1''+v')=R(m+\mu')T_1'''$$

or

$$P_1(V_1''+v')=P_1(V_1'+v')-\frac{1}{k}(P_1-P_0)v'; \qquad (49)$$

V_1' is given equations 38 and 41. Equation 49 gives the value of V_1''.

The inlet valve being closed, the mass of air $m+\mu$ which is at pressure P_1 and temperature T_1''' expands without gain or loss of heat since we neglect the influence of the sides of the cylinder. At the end of the stroke, this volume becomes V_2+v', its temperature T_2 and its pressure P_2. We have then

$$\left.\begin{aligned} &P_2(V_2+v')^k=P_1(V_1''+v')^k \\ &\text{or} \\ &P_2(V_2+v')^k=P_1 \\ &\quad\left\{(V_1'+v')-\frac{1}{k}\left(1-\frac{P_0}{P_1}\right)v'\right\}^k \end{aligned}\right\} \quad (50)$$

and

$$P_2(V_2+v')=R(m+\mu')T_2 \quad (51)$$

Equations 50 and 51 give V_2 and T_2 if P_2 be known, or P_2 and T_2 if V_2 is known; this latter being the volume described by the piston of cylinder B. We have

$$T_2=T_1'''\left(\frac{P_2}{P_1}\right)^{\frac{k-1}{k}}$$

When there is no waste space we have

$$T_2=T'_1\left(\frac{P_2}{P_1}\right)^{\frac{k-1}{k}}$$

As T_1''' is greater than T_1', it results that for a given weight of air passed through the machine, at a given working pressure, that the final temperature of the expanded air would be higher, and consequently the number of negative calories produced would be less than if there had been no waste spaces.

The work is equal to:

$$W_m=\frac{k}{k-1}(P_1V_1'-P_2V_2)+(P_2-P_0)V_2 + \frac{1}{k-1}(P_0-P_2)v' \quad (52)$$

§ 16. In order that the machine should work to the best advantage it is evidently necessary that the air should leave the cylinder at atmospheric pressure, that is, that P_2 should equal to P_0. There ought then to exist a certain relation between the volume of the compression cylinder V_0+v, the pressure in the reservoir P_1 and the volume of the expansion cylinder V_2+v' which may be determined by the above equations. To fix the dimensions of a machine we may assume V_0+v and P_1 as given, and then deduce the value of V_2+v'.

If we make $P_2=P_1$ equations 50 and 51 will become

$$P_0(V_2+v')^k=P_1\left\{(V_1+v')-\frac{1}{k}\frac{P_1-P_0}{P_1}v'\right\}$$

and

$$P_0V_2=Rm'T_2,$$

whence

$$V_2+v'=(V_0+v)\left(\frac{P_0}{P_1}\right)^{\frac{k-1}{k}} \left(\frac{T_1'}{T^0}=\frac{1}{k}\frac{P_1-P_0}{P_0}\frac{v'}{V_0+v}\right). \quad (53)$$

The work is

$$\left.\begin{aligned} &W_m=\frac{k}{k-1}(P_1V_1'-P_0V_2) \\ &\text{or} \quad W_m=\frac{mkc}{A}(T_1'-T_2) \end{aligned}\right\} \quad (54)$$

This value for the work is the same as found in § 7, where no waste space was allowed for; only the final temperature T_2 being greater for the same weight and pressure, the work of the air is less.

The work of the resistance of the machine is then:

$$\left.\begin{aligned} &W_r-W_m=\frac{k}{k-1}[P_1(v_1-v_1') \\ &\quad -P_0(v_0-v_2)]+\frac{k}{k-1}P_0v\frac{V_0-V_1}{V_1+v} \\ &\text{or} \quad W_r-W_m=\frac{mkc}{A} \\ &\quad (T_1-T_1'-T_0+T_2) \end{aligned}\right\} \quad (55)$$

The negative heat produced is

$$Q=mkc(T_0-T_2) \quad (56)$$

$$Q_1-Q=W_r-W_m \quad (57)$$

The performance of the machine is

$$u=\frac{T_0-T_2}{T_1-T_1'-T_0+T_2}$$

or

$$\frac{T_0}{\frac{T_1-T_1'}{T_1-T_1'''}T_1-T_0} \quad (58)$$

As T_1''' is greater than T_1, the useful effect is less than if there had been no waste space.

§ 17. The following table exhibits the results of a machine having waste space of 4 per cent. of the volume described by the pistons. The amount of air used being a cubic meter at 15°, and weighing $1^k.226$. In the cooler the air is brought to 18°.

P_1 (Atmospheres.)	Temperature of outflow from compressor.	Temperature of final outflow.	Cooling of air.	Negative calories.	Theoretical work.	Effective work.	Negative calories developed.			
							Per theoretic kilogrammeter.	Per theoretic horse power per hour.	Per effective kilogrammeter.	Per effective horse power per hour.
	degrees	degrees	degrees	cal.			cal.	cal.	cal.	cal.
1½	51,04	—14,17	29,17	8,492	478	1.150	0,017777	4.800	0,007385	1.984
2	79,31	—34,76	49,76	14,486	1.426	2.582	0,010160	2.743	0,005611	1.515
2½	102,93	—49,17	64,17	18,682	2.563	4 094	0,007290	1.968	0,004563	1.231
3	123,39	—60,33	75,33	21,931	2.711	5.555	0,005910	1.596	0,003948	1.066
3½	141,57	—69,21	84,21	24,516	4.859	6.970	0,005045	1.362	0,003517	950
4	157,98	—76,57	91,57	26,658	5.976	8.321	0,004470	1.207	0,003204	864
4½	173,00	—82,78	97,78	28,468	7.063	9.617	0,004031	1.088	0,002960	799

By comparing these results with those of § 11, we see that the effect of waste spaces is by no means to be neglected since it results in a loss of about 100 calories for each theoretic horse power per hour.

§ 18. We can neutralize the influence of waste space by closing the outlet valve of cylinder B before the end of the stroke, so as to compress the air in this space; the stroke of the piston being exactly determined, the air in the waste space may be brought at the opening of the inlet valve to the temperature T_1' and the pressure P_1'.

In this case the equations 34 and 43 apply without change.

During the period of expansion we have:

$$P_2(V_2+v')^k=P_1(V_1'+v')^k \qquad (59)$$

$$P_2(V_2+v')=R(m+\mu')T_2 \qquad (60)$$

$$P_1V_1=R\mu'T_1' \qquad (61)$$

whence

$$\frac{T_2}{T_1'}=\left(\frac{V_2+v'}{V_1'+v'}\right)^{k-1}=\left(\frac{P_2}{P_1}\right)^{\frac{k-1}{k}} \qquad (62)$$

The work restored by piston B is to make allowance for the compression of air in the waste space from the pressure P_0 to P_1:

$$\left.\begin{aligned} W_m=&\frac{k}{k-1}(P_1V_1'-P_2V_2)-\\ &(P_0-P_2)\frac{k}{k-1}P_0v'\frac{V_0-V_1}{V_1+v}\\ &+\frac{1}{k-1}(P_0-H_2)v' \end{aligned}\right\} \qquad (63)$$

or

$$W_m=\frac{mkc}{A}(T_1'-T_2)+(k-1)\frac{mc}{A}T_2\left(1-\frac{P_0}{P_2}\right)-(k-1)\frac{\mu'c}{A}\left(\frac{P_0}{P_2}T_2-T_2'\right);$$

T_2' being the temperature of the air in the cylinder at the moment compression commences before the end of the stroke.

We have then:

$$\left.\begin{aligned} W_r-W_m=&\frac{k}{k-1}\\ &[P_1(V_1-V_1')-P_0V_0+P_2V_2]+\\ &+(P_0-P_2)V_2+\frac{k}{k-1}P_0(v-v')\\ &\frac{V_0-V_1}{V_1+v}-\frac{1}{k-1}(P_0-P_2)v'. \end{aligned}\right\} \qquad (64)$$

When the machine is well regulated the final pressure $P_2=P_0$ and the equations 63 and 64 become

$$W_m=\frac{k}{k-1}(P_1V_1'-P_0V_2)+\frac{k}{k-1}P_0v'\frac{V_0-V_1}{V_1-v} \qquad (65)$$

or $$W_m=\frac{mkc}{A}(T_1'-T_2)$$

and

$$\left.\begin{aligned} W_r-W_m=&\frac{k}{k-1}\\ &[P_1(V_1-V_1')-P_0(V_0-V_2)]+\\ &+\frac{k}{k-1}\cdot P_0(v-v')\frac{V_0-V_1}{V_1+v}. \end{aligned}\right\} \qquad (66)$$

We have also:

$$\frac{V_0+v}{V_1+v}=\frac{V_2+v'}{V_1'+v'}. \qquad (67)$$

We see that in equation 66 the term relating to waste spaces disappears if we make $v=v'$. The equation then becomes

$$W_r - W_m = \frac{k}{k-1}[P_1(V_1 - V_1') - P_0(V_0 - V_2)]$$

The volume V_2+v' is determined by means of equations 39, 41 and 67 when the pressure P_1 is known.

Reciprocally when V_0, v, V_2 and v' are known (the dimensions of the machine) then V_1' is readily found, and consequently P_1 and T_1, the pressure and temperature at the end of the stroke in cylinder B to insure the escape of the air at the atmospheric pressure.

§ 19. It was remarked in § 5 that the efficiency of the machine could be notably improved by cooling the air in the interior of the compressor cylinder.

This result can be accomplished, in part at least, if not completely, by means of a jet of water, such as is employed in compressed air engines.

We will proceed to calculate the work necessary for the compression in this particular case, neglecting the effect of waste spaces.

Let m be the weight of dry air occupying the volume V_0. Let M represent the weight of water injected together with the amount of moisture in the air, and Mx the weight of the vapor at any instant.

The dilatation or compression of the mixture of the vapor and air is effected in such manner as to satisfy the differential equation:

$$mcdt + M(dq + dx\rho) = -AP dV. \quad (69)$$

which expresses the fact that variations in the internal heat of the mixture equal the variations of work accomplished.

We have also

$$dq = c_1 dt,$$

c being the specific heat of water.

The differential equation can then be written

$$(mc + Mc_1)dt = -Mdx\rho - A(P-p)dV - ApdV$$

p being the tension of the vapor, and P that of the mixture.

But $$x\rho = xr - Apxu$$

$$dx\rho = dxr - Apdxu - Axudp.$$

and $$dV = Mdxu$$

ρ is the latent heat of the vapor,

r is the heat of vaporization,

u is the increase of volume of a kilogram of water vaporized.

We know furthermore that

$$A x u \frac{dp}{dt} = \frac{xr}{T}.$$

We have then

$$Mdx\rho + ApdV = Mdxr - \frac{xrdt}{T}$$

or $$\frac{Mdx\rho + ApdV}{T} = Md\frac{xr}{T},$$

from which we deduce

$$(mc + Mc_1)\frac{dt}{T} + Arm\frac{dV}{V} = -Md\frac{xr}{T}.$$

Integrating between the limits T_1 and T_0,

$$(mc + Mc_1)l\frac{T_0}{T_1} + M\frac{x_0 r_0}{T_0} - M\frac{x_1 r_1}{T_1} + ARml\frac{V_0}{V_1} = o, \quad (70)$$

$$Mx_0 = \frac{V_0}{u_0} \text{ and } Mx_1 = \frac{V_1}{u_1};$$

$\frac{1}{u_0}$ and $\frac{1}{u}$ are very nearly the reciprocals of the vapor densities under the pressures p_0 and p_1.

We have furthermore

$$V_1 = Rm\frac{T_1}{P_1 - p_1}.$$

Equation 70 will give M when T_1 and T_0 are known.

$$AW_r = mc(T_1 - T_0) + M(q_1 - q_0) + x_1\rho_1 - x_0\rho_0) + A(P_1V_1 - P_0V_0)$$

or $$AW_r = mkc(T_1 - T_0) + M(q_1 - q_0 + x_1 r_1 - x_0 r_0) \quad (71)$$

This equation gives the work of resistance when M has become known.*

In M. Colladon's compressors, into which a spray of water is injected, the air being compressed to four atmos-

* The two equations 70 and 71, which express the relations between the volumes and the temperatures of a mixture of air and vapor, which is compressed or dilated, and which determine also the value of the work, are applicable to the Mekarski motor.

In this machine, which is designed to employ compressed air, the air is reheated just before it is introduced into the cylinder by being forced through water, having a temperature of 100° to 150°. The cylinder then contains air and saturated vapor, heated to a mean temperature of 100°.

The weight M of equations 70 and 71 is then the weight of the vapor contained in air, saturated at the temperature at which it leaves the hot water.

pheres, the temperature T_1 does not rise above 50° centigrade, the external air being about 50°.

We deduce then

$V_1 = 0.28429$ cu. metres
$M = 0.57212$
$W_r = 15.291$ kilogrammeters.

When the compression is effected without external cooling, we found in § 11 that the work of compression = 17.649 kilogrammeters, which shows a gain in the above process of about 13 per cent.

It remains to determine W_r for any pressure without any known value of T_1.

When a certain volume of air is dilated or compressed, with or without the addition of heat, the relation of pressure to volume is expressed by the equation

$$PV^a = \text{a constant.}$$

$$\frac{V_1}{V_0} = \left(\frac{P_0 - p_0}{P_1 - p_1}\right)^{\frac{1}{a}} \qquad (72)$$

and

$$\frac{T_1}{T_0} = \left(\frac{P_1 - p_1}{P_0 - p_0}\right)^{\frac{a-1}{a}} \qquad (73)$$

which gives

$$\frac{a-1}{a} = \frac{\log T_1 - \log T_0}{\log(P_1 - p_1) - \log(P_0 - p_0)}; \qquad (74)$$

T_1 having been found by experiment, equation gives a.

Making 74 $P_1 = 4$ atmospheres, $T_1 = 323°$ and $T_0 = 288°$ we find $a = 1.0912$. a being thus determined equation 73 will give M_1. Only p_1 being a function of T_1, the latter must be found by successive approximations.

Equation 70 gives

$$Mc_1 = 0.4343 \frac{\frac{V_0 r_0}{u_0 T_0} - \frac{V_1 r_1}{u_1 T_1}}{\log \frac{T_1}{T_0}} + 0.5888\, m.$$

r_0, u_0, r_1 and u_1 are furnished by the tables.

Finally we obtain W_r by equation 71. The saturated air in passing into the cooler is reduced in temperature from T_1 to T_1', and a portion of the vapor is condensed. The weight of vapor remaining and introduced into the expansion cylinder is:

$$\mu_1 = \frac{V_1'}{u_1'}$$

$\frac{1}{u_1'}$ being the density of the vapor corresponding to the temperature T_1'.

We will calculate again the cooling produce by the expansion and the work as explained in § 13.

§ 20. The following table exhibits the results obtained from a cubic meter of air saturated at 15°, since the sides of the compressor cylinder are covered with water. The weight of the air is 1^k 021.

Pressures	Temperature in compressor.	Temperature of air going out.	Reduction of Temperature.	Negative calories obtained.	Theoretic work developed.	Work of resistance.	Effective work developed.	Negative calories obtained.				Weight of vapor introduced into cylinder B.
								Per theoretic kilogrammeter.	Per theoretic H. P. per hour.	Per effective kilogrammeter.	Per effective H. P. per hour.	
	degrees	degrees	degrees	cal.				cal.	cal.	cal.	cal.	
1½	24,79	+ 5,79	9,21	2,636	189	4.266	856	0,0139	3.763	0,00308	832	0,01010
2	32,00	−18,62	33,62	9,623	709	6.687	1.833	0,01357	3.663	0,00525	1.417	0,00759
2½	37,68	−37,27	52,27	14,962	1.368	9.885	2.953	0,01094	2.953	0,00527	1.423	0,00609
3	42,38	−51,30	66,30	18,979	2.000	11.940	3.750	0,00949	2.562	0,00506	1.366	0,00509
3½	46,26	−62,48	77,48	22,179	2.636	13.723	4.621	0,00841	2.270	0,00480	1.296	0,00436
4	50,00	−71,60	86,60	24,790	3.244	15.291	5.431	0,00764	2.063	0,00456	1.231	0,00382
4½	53,13	−79,40	94,40	26,022	3.809	16.672	6.172	0,00709	1.914	0,00438	1.182	0,00340

An examination of this table and a comparison with the table of § 14 shows:

1st. That the injunction of water into the interior of the compressor cylinder increases the efficiency 40 to 50 per cent.

2d. That the efficiency is at a maximum at a pressure of $2\frac{1}{2}$ atmospheres.

3d. That it diminishes, though slowly, as we vary from this pressure.

4th. That the quantity of snow or ice produced is not greater than that which comes from the moisture of the atmosphere.

The most favorable working pressure apppears to be in this case nearly 4 atmospheres, since we obtain then a sufficiently good result (24 to 25 negative calories for a cubic meter of air), with a relatively good performance of 1,200 negative calories per horse-power per hour.

Theoretically the injunction of water into the compressor affords a great advantage. But it is possible that the water resulting from the condensation of vapor in the cooler does not all remain in the reservoir, but that a portion is carried mechanically in cylinder B.

The results indicated above for the efficiency would in such a case be considerably modified, and the increase in the quantity of frozen vapor would constitute in practice a grave inconvenience.

Experiment can alone decide this question.

We have examined in the preceding pages nearly all the problems belonging to the air machine. We will pass now to the study of the second class of machines, or those which transform motive force into negative heat by the employment of a liquefiable gas.

BRIDGING THE INDUS.—Not long since, in an article on Indian Frontier Railways, we drew attention to the dilatoriness with which the extension of Indian State Railways had been carried out; and the difficulties which had in consequence to be overcome in forwarding the supplies to the army now engaged in Afghanistan. One of the most formidable of these was the crossing of the river Indus, which now practically cuts off railway communication from the most outlying of the north-west provinces of our Indian Empire. There can be but little gained by extensions to the north of this river until a bridge is constructed to connect the lines on its southern and northern banks. This fact has long been foreseen: but notwithstanding the obstacle presented, by the want of a bridge, to efficient extension of the Indian system of State Railways, the various proposals which have from time to time, for years past, been submitted to the Indian Government for carrying out the work, have been always shelved and committed to the limbo of good intentions. The Indian authorities have feared to commit themselves to the outlay which the undertaking must necessarily involve, and their procrastination, whether justifiable or otherwise, brought them face to face with a serious obstacle when the Afghan war broke out, which forced upon them then an expenditure far greater than would have sufficed to construct the bridge over and over again. Taught by their recent experience, however, we learn that the Indian Government has decided upon undertaking at last this important work, and Mr. Guilford Lindsay Molesworth, M. Inst. C. E., the Director of Indian State Railways, has received instructions to submit designs for the structure. We understand that Mr. Molesworth purposes to cross the main stream by a single span of 750 ft., and that in its construction he contemplates the use of steel only. Experience in the use of this material for such work being as yet comparatively limited, the Indian Government, we are informed, considered that, before committing itself to final acceptance of Mr. Molesworth's designs, it would be most desirable that that gentleman should proceed to England and make himself fully acquainted with all the results of the most recent practice with it, and with such details as to its manufacture and testing as should enable him to be able to specify, with confidence, the method in which the work should be carried out. His design of such a span as 750 ft. is undoubtedly very bold, and is not exceeded by any girder structure yet erected in any part of the world. Those responsible for the acceptance of the design have therefore done wisely, in affording Mr. Molesworth every possible facility for guarding against failure by previous study of the best examples of steel manufacture and construction open to him in England and on the Continent. The enterprise is a novel one, and will open the road to the future development of the use of this material, which, although as yet in its infancy in such forms of application, promises to be *the* material of the future. Its employment, owing to its light scantling, will render novel and special forms of construction necessary to guard against the tendency to buckle, likely to manifest itself in such a great span as that of the proposed girder.

DURING the considerable period of trial, error and success which has marked the history of the application of iron to the permanent way of railways, English railway chiefs have rejected proposals to use iron on their roads. Iron sleepers and fastenings have now arrived at something like a practical form, and consequently the North-Eastern Company is trying Mr. C. Wood's system, and the London and North Western Company is about to make a somewhat extensive trial of the rolled channel iron cross sleeper.

ON THE IMPORTANCE OF A SPECIAL ORGANIZATION FOR THE DIFFUSION OF SANITARY KNOWLEDGE.

By Major-Gen. SYNGE, F.R.G.S.

From "Journal of the Society of Arts."

A self-evident proposition is admitted to be difficult of proof. An axiom may be stated, but is supposed to be incapable of demonstration, and the attempt even to reason on the difficulty of substantiating logically that which commands instinctive assent, leads at once into the region of metaphysics. I should venture on yet more subtle ground were I to hazard entering upon the inquiry why self-evident propositions or truisms stand pre-eminent among the things universally neglected in practice. Yet my subject, viz., "The Importance of a Special Organization for the Diffusion of Sanitary Knowledge." places me in this difficulty and takes me into this region. For when I have stated it, shall I not have enunciated a self-evident proposition? Who is there that will dispute the value of knowledge, provided only it be real? Or who will deny the value of health? Who will question that the one bears upon the other? Health cannot be maintained without knowledge, whether that knowledge be instructive, intuitive, or acquired. And who is there to be found bold enough to affirm that our existing habits in matters relating to the maintenance of health are satisfactory? If there be such an one, I should select him as the strongest and most conclusive embodied evidence it were possible to procure, or to adduce, of the necessity which exists for the diffusion of knowledge leading to a better condition of mind. If, on the other hand, there be none such, my proposition is in great part admitted, and, in so far, my object attained. If the habits of a great proportion of our population be in many respects not conducive to, but subversive of, the due maintenance of health, who will gainsay the propriety of, if there exists not the absolute necessity for, a special organization to spread sound knowledge on sanitary subjects; but an organization armed with that authority which is derived from conviction brought to bear rather than with that comparatively unreal and unstable force which may indeed be procured by the instrumentality of legal enactments, but which is never willingly submitted to when enacted contrary to conviction, and which never carries, and never can carry, the same inherent power as stands inseparably connected with settled conviction founded upon the knowledge of the true; and this is, to my mind, the sound definition of science—science is true knowledge of the true. In accordance with these views, I seek, in bringing this subject under your consideration, to arouse the energy of action, so that it may be applied directly to the performance of this task, and this Society be moved to form a distinct branch, which I hope will cover the land, and which shall base its action on somewhat of the following principles and outline of suggestion. First, on patient and continuous investigation of what is true in that class of subjects pertaining to health which is usually ranged under the term "Sanitary;" next, on clear and concise compilation of the evidence on which each step in such investigations shall rest, and which shall proceed to diffuse or spread such knowledge, but which shall leave entirely free and unfettered by any recommendation of legal enactments the adoption or the non-adoption of the results which may seem to flow from the establishment on such evidence of the premises in question. I shall presently seek to direct your attention at somewhat greater length to the grounds on which I press this advocacy of perfect liberty; but I introduce it thus early to your notice because, for my own part, I am as thoroughly satisfied of the policy, as I am of the propriety, of limiting legal enactments to such negative injunctions as fetter liberty only where it degenerates into license, and of never attempting to command an individual or a community to tread in some particular positive path. Particular enactments, however much they may possibly be deemed right at the time, may all the while be wrong;

but this is the least evil aspect of the question. The intrusion of legislative or government direction beyond the function of maintaining order tends to unman a people, and is both a cause and symptom of decline. This, even if the legislation and direction be right in themselves; but the legislation of wrong is the violation of law, and treason against its majesty. Law forbids our injuring our neighbor, but law is powerless in the direction of direct benefit. We cannot by any means whatever do any positive good to our neighbor by means of a legal enactment. We can and we ought to abstain from doing our neighbor harm, but every needless, and, much more, every injuring enactment is organised wrong to our neighbor, and wrong in the worst form. The only enactment I should desire would run in ordinary phraseology enough; it would be an Act to amend by repealing other Acts. The necessity of combating at every turn the love of forcing our shibboleths on others has led me into this necessary digression; but returning to the direct means I would suggest for diffusing sanitary knowledge, I would propose that the Society of Arts request the Council of the Annual International Exhibition to give permanence, and prominence together with that permanence, to that sanitary, architectural, and engineering department which forms so instructive, timely, and valuable a feature this year. Also that it should invite the Council of that Exhibition to cause lectures to be delivered by the exhibitors or others, bringing forward, with all the advantage to be derived from the examples at hand for inspection, their own views in explanation of the works they have produced.

I have jealously guarded against the intrusion of compulsion by means of legal enactment, although it is the pet abomination of the hour; but I would also guard against being supposed to be content to begin and end with the mere machinery of exhibitions or lectures either there and here. I have a very different aim in proposing the formation of this important branch of the Society of Arts. I believe that with very rare, if with any, exceptions our scientific institutions fall short of exercising the influence and carrying the weight they might and should, if they followed more invariably the rule of bringing to a practical conclusion and pronouncing a definite judgment on the questions brought before them, and formed sub-committees charged to see approved conclusions carried into practice by science, that is to say, by sufficiently diffusing sound knowledge on the particular subject. And this is what I propose that we should do: A geographical problem, for instance, unfollowed by an expedition, is of limited value. It would have still less if not even an opinion on the value of the problem is authoritatively pronounced. Let me give you two striking examples, which sufficiently illustrate what latent force is wasted by that neglect. It is not at all too much to say that had the course I advocate been adopted in these instances, the whole political aspect of the world, and the distribution of power, would have been different from what they are. Communication from ocean to ocean over the British territories of the American continent, federating the whole empire in a defensive and commercial league, would have been a work accomplished many years ago; the Suez Canal would have been an English, not a foreign enterprise, and Russo-Persian railways would not have antedated the Euphrates Valley line. Again, were there not sufficiently apparent obstacles in the way, the valuable papers brought before the Royal United Service Institution, and the yet more valuable discussions they elicit, would have a very different effect were they, or could they be followed up by committees of that body charged to investigate and to support or to refute the theses brought under consideration, and charged to bring the result of their deliberations publicly before the Administrative Department. How different under such conditions might be the strength and the efficiency of both our gallant services, and by how much diminished that magnitude of the Taxpayers' Bill, which has led to the deplorable and dangerous result of the arms of the service and consequently of the country being characterised, not in satire nor in wrath, but as a supposed matter of fact, as the spending departments, till the defence of the country has thus practically fallen into the care and custody of accountants. Happily any difficulties which may stand in the way of the course which I pro-

pose, were it suggested in the cases I have just given as illustrations, do not at all exist for us here. I have adduced these illustrations hoping that you will think of them again and again, till they have helped to impress indelibly upon your minds the immense but unsuspected power latent in manly effort, through associated independence and its immense superiority in attaining result over anything that can possibly be brought about by any other means. In spite of an Act with some Latin words hard to peasant understanding, which has substituted "urban" and "rural" for "town" and "country;" and which has done very little else, there is no department of the Government charged with decreeing and determining what is conducive to health; but if there were we should be wholly free from any special allegiance to it. Our freedom, however, in this matter is not merely complete. We are actually invited by the circumstances of the case to take up the ground which has been left clear. I fear, not from any intelligent understanding, that it is wholly beside and beyond the province of administrative government to decree and determine what shall be the laws of nature which are to govern bodily health, and that it is a proper function of administrative government to cause such laws, which are are a part of nature, or, in other words, inherent in the creation, to be discovered and recognized. I say it is less, I fear, to any clear apprehension of this fact, that we are indebted to our freedom in the matter than it is to the hopeless perplexity of minds which have traveled in the groove of evolving legal enactments out of their inner consciousness, and which, when they look to that inner consciousness for sanitary light, find nothing and evolve that. I think I shall sufficiently establish this statement by a single illustration. Legal enactments, at the present moment, compel the introduction of an enormous bulk of putrescible matter into the fluid we drink and are supposed to wash in. That fluid is thereby itself made putrid. It consists of gases which, in contact with putridity, change their qualities, and evaporate in new and pernicious formations. The bulk of the fluid remains more or less putrid. These legal enactments again step in and forbid the fluid in that condition to remain in the river bed. The victims of this double compulsion meekly ask what they are to do, but the inner consciousness has exhausted its resources, and gives neither voice nor answer. It remains an utter blank. The victims find themselves the mere sport of a power which commands them under irresistible penalty to bring about a state of things which the same power declares makes water unfit for river beds. First, the adoption of a particular mode of attaining a certain end is made imperative, and next, justifiably enough, except for this previous circumstance, it is declared that poisonous water, or water in a condition calculated to do injury to health, shall not be poured into river beds; but when it is asked of administrative government, which is answerable for this state of things, "What are we to do?" there is given first the reply, "repurify," but to the farther question of "How?" the response is like those echoes of Killarney which add to the sounds they reverberate, and the answer is, "Anyhow! we neither know nor care." To suppose, however, that legal enactments which result from what the present Prime Minister is peculiarly apt to call "the wisdom of Parliament," or "Parliament in its wisdom" stop here, would be to supppse an error. Simply to forbid poisonous water, or water that is in a state injurious to a sound state of health, being poured into river beds were a desirable, if not an absolutely necessary rule. It would be a rule in consonance with law in its right sense, that is to say, in harmony with right, morality, and reason Legal enactments have found their way out of such a dilemma as this which would have involved them in a state of things foreign to their customs. The mode of escape was simple and indescribably efficacious. They did not *proscribe* any measured degree of *corruption*, but they *prescribed* a standard of *purity*. It answered every purpose. It sounded magnificently grand and lofty. It was not evil that was to be forbidden, positive good was to be ordered, and purity in water was to be established by Act of Parliament. The first question, however, was, "What is purity in water, and what is its standard?" To determine the order of the universe seems to exercise an irresistible fascination over the framers of legal fet-

ters. Here an exceptionally fortunate opportunity for the indulgence of this morbid fancy was presented. It offered first an illimitable field for every sort of wrangling. Chemists, lawyers, legislators, each and all could have their say, and all help to the one end of avoiding practical simplicity through having raised a thoroughly non-pertinent issue. The success has been complete. The introduction of a so-called standard of purity instead of the prohibition of defilement has left the determination of that standard undetermined, and it has left the great water arteries of the country in the evil state in which it found them. The "standard of purity," so declared the "Rivers Purification Committee of Scotland" composed of dukes, marquesses, and other peers, and baronets, and of merchants, manufacturers, and proprietors of Scotland, who associated themselves together to end the pollution of their waters, "The standard of purity would destroy the manufactures of the country." On the other hand, the manufacturing town of Bradford, in Yorkshire, has been under injunction to prevent its pouring its pollution into a stream much fouler than the Bradford flood. To enjoin and to forbid, to puzzle and perplex, these are the results of legal enactments, and such appear to be their revelry and their delight. So far as the limits of time allow me with regard to other points I must bring before you, I dwell at some length and as impressively as I can upon this point, because I believe it to be the very root, not only of many social evils under which we suffer, but I believe the fondness for legal enactments and the blind credulity that puts its trust in them, are threatening us with some danger of that hopeless and despicable condition in which the sense of manhood has died out. It is a lazy, cruel, and immoral thing to thrust upon others our views or our interest through the intricacies of a tyranny which is not at all the less real for being cloaked by hypocrisy and tempered by folly, and although the embodiment of this evil is in the enactments themselves, these only come into being through the operation of that feeling which seeks more and more to place all life under government rule and supervision, instead of jealously guarding against every encroachment on individual liberty, and every form of usurpation over moral law. The charms of compulsion when applied to others seem daily more and more to deaden, till they threaten to obliterate the sense of morality and the power of distinction between right and wrong, to extinguish the very love and sense of liberty, and that jealousy of injustifiable interference, both the effect and cause of the former manliness of English character, and the foundation alike of the past greatness and the present prosperity of the country. "Government without a Parliament," said the great Lord Burleigh of the reign of Elizabeth, "is an object of terror, Government with a Parliament is an object of desire;" but he added, "England can never be undone but by a Parliament." Government by Parliament he described as an infallible mode of compassing the ruin of the country. Even that prescient statesman, however, has left no record that proves he foresaw that we should delight in being poisoned by Act of Parliament.

A man of modern times, not his inferior it may be in wisdom, has said—"If we adopt the recent English idea of factious contention as the meaning of politics, no man can be a politician and a Christian; but if politics be the knowledge of our duties as citizens, there can be no Christian that is not a politician."

The remedy then, for the grievous abuse and injury which result from unjust, ill-considered, and ever mutually destructive legal enactments, is to be found in the diffusion of knowledge and the performance of our duties as citizens in respect of cleanliness, and in the cultivation of character and manliness, just as on the contrary legal enactments have their root and origin in the indolence of inconvenienced incapacity. It is to press upon you to apply and energize in a direction in which they are very much required, the self-help and the capacity at the command of this influential and able voluntary association, that I bring this subject before you. I do it the more earnestly because the love of compulsion is a very prevalent, as well as a very dangerous disease. Men vary in opinion on many matters; but there seems a dangerous agreement in the desire to "compel" their neighbors to act upon their individual or sectional convictions. This

emulation can end only in destruction. This perpetual appeal to power acts as an incentive to singularity of opinion, and the truant infant that began life by knocking over his companions, grows into the angry legislator who thrusts his nostrums or his self-will upon his fellows, enrolling them in Acts of Parliament, and labelling them "with compulsion." On some sanitary questions I have found myself not in consonance with the majority—at least it was so at one time—but I found myself confronted with an evil greater, in my apprehension, even than poisonous waters; those who shared my convictions on these matters shared those of my antagonists as to the means of giving effect to convictions, and adopting the precept of evil for evil, wanted to make running in the arena of factious contention, and to meet the legal enactments of their adversaries with others of their own.

In passing on to the consideration of the subjects that come within the category of sanitary science, I come to a branch of the subject with which I feel that it is quite beyond my power to deal exhaustively, I mean so comprehensively, as to include satisfactorily in general terms its wide and multifarious ramifications. It is imperative, nevertheless, that I should venture at least to indicate those prominent parts of sanitary science of which I have no hesitation in affirming that the general knowledge and general observance are alike imperative and indispensable to the maintenance of the health of concentrated populations in an unvitiated condition. And here I feel also that the task is very much lightened if we continue a due regard to the principle I have already submitted to you, namely, that associated efforts in material subjects, as well as legislation on social matters, should be restricted to negative conditions, but that in this direction the one should be vigorously exerted, and the other be rigidly enforced.

If we accept this principle, and proceed to base the outline of sanitary effort upon it, we shall at once be brought to these conclusions; and first in regard to the atmosphere. Its normal state should not be unnecessarily made worse, and never so except upon sufficient cause shown. It follows as a logical sequence that between rival pretensions to deal with that law of decay, of which putrefaction is an inseparable part, that which *cæteris paribus* prevents such decay or putrefaction, or other deterioration, from so much as reaching or affecting the atmosphere, is to be preferred. Should there be none such, the next best, in the abstract, is evidently that which most nearly approaches to prevention, and most effectually remedies such as may be inevitable. The question of cost is in principle altogether subordinate. It only enters as a matter of detail. There and then, however, it may assume commanding proportions. For instance, the case may be conceived in which an injurious taint of the atmosphere may inevitably and inseparably attach to a particular manufacture. On the other hand, it is quite conceivable that the manufacture characterised by this defect may be of very great importance, and the locality in which it is carried on might be one in which the injurious taint was all but confined in its effects to those conducting the manufactory or directly benefiting from it. Moreover, the actual state of science as to repurifying the atmosphere from the taint might be at a low ebb. There might possibly be no particular remedy whatever except at a cost and by exertions that would prove fatal to production; or if there were one it might be unknown or undiscovered, which amounts practically to the same thing. To lay down a hard and fast rule made to apply alike, and under similar enactments to such vitiation; and to vitiation, wanton, vicious, and preventible, would be exactly what our legal enactments have done, and pretend to do, and affords but another illustration of the mischief in practice, as much as in principle, of that fond resort to Parliamentary fetters and hand-tying which, for my part, I deem a worse remedy than almost any evil against which it may be directed.

The tribunal I invite you to form would be wholly free from temptation to fall into such errors. Its mission will be to enlighten; it cannnot legislate. Its object primarily would be to attain and to spread "true knowledge of the true," and it would be guided throughout by deductions derived from impartial and patient investigation. It would ascertain and make known the circumstances attending upon each subject which it

investigated. Whilst, therefore, taking the very humble but all-powerful position of being strictly bound to ascertaining what is fact, and absolutely hindered from pretending to determine what wishes, theories and pretensions can be converted into facts, it would embrace a scope and freedom of application at the present time, unhappily, utterly unknown among us, and, moreover, altogether unattainable until we substitute for the arrogance of legislative decrees the humility of the observance of facts.

Identically the same principle applies to the next indispensable to life, water. In lieu of a vague, imaginary, and utterly inapplicable prescription of a visionary standard of purity, the negative law would evidently lead to a proceeding as feasible in practice as simple in enunciation. Intense absurdity when clothed in the form and invested with the force of legal enactment is tyranny and cruelty, as well as absurdity; but the intense absurdity, I say, of compelling water to be mingled with putrescible matter, and then forbidding the corrupted mass to flow into the channels of the river beds without providing any remedy, is sufficiently apparent. I propose, therefore, that this Society, which undertakes to deal on the basis of true knowledge with the interests of science, art, and manufacture, should take up with a vigor and earnestness proportioned to the magnitude of the interests at stake, and should render impossible the continuance of this condition.

In the face of the tribunal I invite you to inaugurate, and which it is in your power to form effectively, no manufacturing town could have its industry laid under that form of interdict our legal courts term "an injunction," whilst the legislative body, occupied with party strife and with conundrums for the compulsory propagation of knowledge as it is defined by Act of Parliament, and an administration entirely bewildered, look on in helpless bewilderment, ignorant of the ignoble part they play in laying down an eternity of enactments on matters they are too indifferent and careless to investigate.

Again, in contrast to this condition the negative law would merely enjoin that water which might be used (or abused even) in any manner any one saw fit, should not leave the premises or the city in a worse condition than it reached the householder or the city. This furnishes the solution supplied by reason, and is as applicable to a single householder as to a city. Leaving our municipal institutions intact, it would enable the authorities in such matters to determine on any rules that they saw fit, both as to atmosphere and water, so long as they come within these limits, whilst at the same time they could work little or no positive ill to their neighbors.

Precisely the same principle holds good as to land. I shall not enter into the subject of the ownership of land; I will take it as I find it. Our legal exactments, be they wise or not, enable land to be taken by the community in its supposed interests. I am free to own that I think the judgment, moderation, and full compensation usually shown and accorded, when this legal right is exercised, are among the most encouraging and gratifying evidences of the deep good feeling and thorough honesty of purpose, which I am glad to believe underlie any errors into which we may fall from that rapidity of life and multiplicity of avocation from which we cannot now separate ourselves even if we would. Thus, while I should invite the Society to form a Sanitary Section, and ask the Society itself through it thoroughly to investigate and thoroughly to test all uses of land which might pretend to sanitary result, but of which the sanitary result might be called in question, and fearlessly to deliver and support the conclusions it may arrive at, I would also invite it scrupulously to leave all action on such conclusions altogether to the municipalities and land owners of the country.

I may, however, as well forestall or endeavor to forestall, an objection which may very possibly suggest itself to some by no means uninfluential members of this Society. I am aware that an opinion is in some quarters entertained that we, or at least that certain parties are in possession of all knowledge, now or ever to be attained, on sanitary matters. Those who think so point with emphatic satisfaction to our actual state, reasoning not unnaturally from their own point of view, that these their convictions need only to be acted upon in order to produce that

which we all desire, namely, the most perfect sanitary condition which it is possible to attain. This opinion is held, and very stoutly maintained, by men whom I have called not uninfluential. They are so far from being uninfluential, that the great metropolitan area, in a microscopic fragment of which we are asembled, has been brought under the pains and penalties with which, in their fondness for enforcing their views by the instrumentality of legal enactments, they grace their convictions. I shall not question the soundness of those convictions on this occasion. I do not think it the occasion on which to express any opinion beyond the one opinion that forms the all-in-all of the object of the present address. I desire to concentrate your attention on the value of an association which shall spread its influence by means of evidence made universally accessible and plainly demonstrative, and that shall hate pains, penalties, and compulsion more than we do poison. In that spirit I invite the co-operation and support of the persons I refer to as cordially and as earnestly as I do that of all others; and what I beg of them is, not that they should throw away their convictions, but that they should trust to the force and value of the evidence on which no doubt they believe those convictions to rest, and that they would break off the miserable fetters with which they have been the means of enslaving the volition of all the millions inhabiting the metropolitan area. I wish them to believe in the free trade—if I may avail myself of a cant term to convey my meaning rapidly—in the free trade or interchange of knowledge, and in the right of every man to pursue any course he pleases, so long as that course inflicts no injury to his neighbor. I must, however, prepare to meet a farther objection. I may be told that my principle is a theory, or that it is impracticable, or—shall I put it in plain words—is nonsense. I might be told that it is impossible for an individual householder to deal with putrescible matter of the various kinds incidental to our existence without offence and injury to his neighbor; that no new tribunal whatever is necessary to determine a self-evident fact, and that, moreover, a fact which the Legislature has recognized after mature consideration; and that the panacea exists already, and needs only to be universally and compulsorily applied. Surely the general dissatisfaction with the existing state of things affords an irrefutable reply to so wide an assertion, however confidently it may be affirmed. Were it possible for a moment to assume the premiss, is it not evident that the opinion that the premiss is sound is not so generally received as to be acted upon? It is impossible almost on any day to take up a newspaper that does not contain some reference to the miserable and dangerous conditions under which we carry on an existence needlessly endangered. Now it is a hospital that has to be reconstructed; then an epidemic spread by means of what it is customary to call milk; next we have an account of cabs alternating between loads of infection and persons to be infected; then contaminated bread; then towns under injunctions—rivers resembling nothing so much as sewers; and so on. Yet in all this category we touch but one form of the evil—for the sin and misery of over-crowding is not so much as noticed. I confess I feel almost overwhelmed by the importance and magnitude of the subject I have ventured to touch.

I would draw your attention to matters of health, and find myself inevitably pleading for everything that can give life value. I cannot keep clear of the moral importance of the subject. I have been told that dirt, over-crowding, drunkenness, and the unnatural fouling of water do not produce an aggravation of the death-rate, even when combined in all the concentrated intensity with which they rage in London. Singularly enough, though I am not at all prepared to accept the conclusion, I had myself, before I was so informed, attributed to a coincident neglect of the primitive religious obligation of cleanliness in all relations of life, the class selfishness, the party strifes, the filthy clothing, and the foul habits which disgrace so huge a proportion of our population. I am almost afraid, even in this assembly, of being deemed unpractical if I set before you the power of true knowledge as much more effective than the instrumentality of a code of rules; but my effort is directed precisely to-

wards this object. The glory of man is dependent on individuality, and we can do nothing aright if we ignore this. We may get work done and we may create misery; we may degrade man and we may foster vice by treating him as droves of cattle are treated; we may rule and we may regulate him; we may render him almost an automaton or animal, but we can never by this means elevate him, never benefit him; we cannot take a single true step towards the lasting prosperity of the nation except by bringing home to him his own individuality, and his personal office in the daily duties and functions of his life. I hope you will not deem this a digression. On the contrary, the facts that constitute the danger of its being so regarded constitute the huge barrier in the way of all real sanitary amelioration. If once you realize that man's duty is to work, but that it is his privilege and his right to be clean in his person, clean in his clothing, clean in his dwelling, clean in the clusters of dwellings, I make bold to say the day is won, the victory secured, the triumph of science certain, for it will be carried out in action.

And now I will venture to add but one consideration more upon this head. I, for my own part, am persuaded that cleanliness is economical and filth the foulest wastefulness. I will put this very generally. Take the instance of smoke, which I have not touched upon as yet. In a country teeming with manufactures, compelled by reason of its low temperature to maintain an exceptional amount of domestic fuel consumption, and possessing a fuel which improperly applied is peculiarly dirty, no arrangement could well be devised more wanting in cleanliness than suffering the perpetual escape of falling smuts sullying, smudging, spoiling everything they touch, and lodging, nestling fixedly everywhere. Our houses are built of rough-surfaced material, giving all possible harbor to this dirt. But all that dirt is fuel wasted, the product of misapplication, destructive to substances, and more or less prejudicial to health. Again, millions of tons of friable material are carted at enormous costs into our streets, to be ground into powder by our horses, our vehicles, and our feet. Whilst undergoing this process it is impregnated with animal manure, then wafted round about ourselves and carried into our rooms in clouds of dust, or it is watered into the consistency of mud and carted off again at a repetition of the original cost. We underlay our streets sewers, drains, connections, gas and water pipes, all without method, system, or protection, and so add as much as we can to the danger, cost and inconvenience that we can accumulate. When rain falls we are in sore perplexity how to carry it off in subterranean rivers, yet we cart from a distance every drop of water that is used to prevent our being literally choked with dust. We employ water in a manner of which it is at least alleged that it cannot be justified in reflection, yet one of the most recent prominent acts of this Society has been to point out, in the most forcible manner that it can, that the whole metropolis is in hourly peril of conflagration by reason of a scarcity of water. We know that under the most favorable circumstances town air cannot rival the mountain breeze in purity, but we construct a laboratory of foul gases, and distribute the outcome to freshen the atmosphere of towns. We have abundant evidence that water is the most effective known means of spreading certain phases of disease. Is it really on that account that we have rendered compulsory the immersion of the putrescible outcome of the largest and most numerous hospitals in the kingdom, those of the metropolis, into water? We know that all putrescible matter forms the natural fertilizer of and is a necessary addition to earth. Is it really on that account that we decree by legislative enactment to throw it into water, by which that value is lost? Now all these acts, unjust as well as silly, arise out of the contempt of individual right and the neglect of individual duty. They all proceed from the substitution of injunction of a means, and of direct action, for the negative commandment of working no ill or prejudice to our neighbor.

I have stated that I feel it impossible for me to deal at all exhaustively with the ramifications of the subject I have brought before you. My difficulty is two-fold. First the abstract difficulty of even enumerating the many causes at work injuriously to affect the air around

us, and our water supplies, and the earth on which we tread. Secondly, I desire especially to avoid on the present occasion falling in any way into the error I condemn, that, namely, of assuming any particular modes of dealing with any portions of these subjects to be perfect, and then pronouncing it to be so. My object, on the contrary, is to induce not only the fullest and most convincing process of investigation possible, but also that the force of influence which such investigation may tend to give to particular methods may be diffused as widely as is possible. I should, however, deal with even this province very unsatisfactorily and imperfectly if I failed to give you any evidence whatever of the wide divergence of opinion that now prevails, or of the comprehensive range of the questions to be dealt with.

Let me then adduce the example of a single dwelling placed in the area occupied by a concentrated population; that is to say, I take as an example a house in a street in a town. I have spoken of air, water, and earth, but at once another indispensable requirement intrudes. There must be heat. Beginning at the basement there are two alternatives, radically contrary the one to the other, that first suggest themselves. Shall the basement contain furnaces or drains? Shall it be underlaid with heating apparatus, or with encased liquids and gases; dry or damp? Endangered or absolutely safe in respect of gaseous emanations and foul smells? Shall the heat be diffused by insensible and active or by present and inactive arrangements? Shall there be fireplaces in each room and chimneys, or an equable heat and one chimney? Shall the exterior be washed or not? Of porous or impervious material? Shall putrescible substances be thrown into water, or shall other arrangements be adopted? If the former, how is the putrid fluid to be dealt with? In bulk, or on the premises? If the latter, what are the arrangements to be? Shall house slops be cleansed in bulk or in detail? Shall all slops be thrown together, or be collected according to their characteristics? Shall each house, if not each room, be dealt with separately and distinctively, or, is it, under certain conditions better to deal with blocks, rows, and houses in the aggregate, both as to warming and waste waters? If water be not employed as a carrier, how are substances usually conveyed by water to be removed? If it is employed, by what means is its repurification to be attained?

Then as to a street. What is the best way of disposing of gas and water pipes and telegraphic wire so as to avoid perpetual tearing up? Which is the best pavement for foot-passengers? What is the best substance for a roadway? What are the proved characteristics and the known cost of each foot of roadway material? Is it desirable to consider a separate material for equestrians as much as in recent centuries footways have been introduced? Are trees and shrubs so beneficial as active sanitary agents that their introduction should be looked upon as desirable wherever possible? Should the rain water which falls upon the streets be collected through street filters and be elevated into reservoirs for purposes of street and exterior house washing and watering, for fountains and other purposes, or be hurried away in a conglomerate of mud as hastily as may be possible? Is dust, the dry refuse of impregnated mud, injurious to health or not? Is it preventible by other means than mud puddling?

These are but samples of the simplest form of questions that arise, and, with the reiteration of my conviction of how very small a portion of the subject after all they touch if I were to offer them as exhaustive, I am compelled to leave them. I have not so much as touched on overcrowding, which is a moral rather than a structural evil. Still I may safely say, that what I have brought forward is important and I have only to show you farther how wide is the divergence of opinion that is now advanced in regard to some of the questions.

In opposition to the optimist views to which I have referred, I will first take at random the expressions of opinion recently delivered when, on the invitation of the Council of the Royal United Service Institution, I invited the attention of that institution to some suggestions for sanitary improvement in barracks, camps, and hospitals. The chairman, Surgeon-General Mouatt, V.C., C.B., said, "For my own part I cannot see any practical solution." It would be difficult

to adduce testimony more decided in favour of the formation of the organization now proposed to you. Surgeon-General Gordon, C.B., said, "It strikes me that we have not quite arrived at that state of knowledge when we can bring forward our opinions as definite. When infection enters it is too late to apply disinfectants. There is an infection that is communicable by water, there is an infection that is communicable by air, and there is an infection that is communicable by both air and water." Col. St. Leger Alcock stated of a poor-law union, "At night, after the doors were shut, the bad air [? ventilation] that came up through the sewers was perfectly intolerable." He added, with regard to a lecture on sanitary subjects, that a poor woman said, "I have lost eight children. Had I known what I have heard this day, I do not think I should have lost one." Referring to the small-pox hospital at Hampstead, he further said, "The objection that it would bring the disease into the neighborhood was looked upon as almost frivolous; but it did. There was communication through the sewers between the houses and the hospital."

Colonel Murray, late of the War Office and Home District, and now commanding Royal Engineers in Ireland, limited himself to saying he was at his own wit's end. He is, I am persuaded, a very fair sample of many thousands ready to welcome the operations of the organization suggested to us, if only they be adequately conducted. He did say, explicitly, "I shall be very thankful if anyone can give a plan which may be applied in practice." On the other hand, Mr. R. Rawlinson, C.B., took a completely opposite view. On the occasion in question, I advocated the agency of fire, after the application of charcoal, for the destruction of germs of infectious disease. Mr. Rawlinson expressed his views at considerable length, and I understood them as totally adverse to that opinion. I also understood him to say that the subject was exhausted, and all knowledge attained. However this may be, he stated that he had "never heard more fallacies crowded into the same space of time;" that water did not give off impurity by evaporation to an injurious extent; and he added that London with its lanes and alleys, and houses six and seven stories high, crowded from top to bottom, life of the least possible value, health with the least possible chance, inhabited by gin-drinkers and foul livers of all kinds, had a death rate of only from 22 to 26 per 1,000. So impressed with the fallacies of the paper and the danger of any weight being attached to it was Mr. Rawlinson, that he closed his remarks with this singular sentence, "The paper is not one I can accept, and, in fact, if I did accept it, I should so incriminate myself that my next business would be to go and jump into the Thames and drown myself." Surely this is carrying partiality for Thames water, even in its present condition, too far. He has added to his remarks made at the time the following note in the Institution's *Journal*, "General Synge said that fire would alone destroy the germs of disease; but an ordinary fire will not do this. Experiments have shown that a considerable percentage of gases pass into, and through, a furnace fire, unconsumed up the chimney, and to the open air."

There is another strong evidence of the value which will attach to our proposed organization, and to the necessity for it. I do not mean merely rescuing Mr. Rawlinson from the fate he somewhat needlessly invoked off Barking Creek, but I refer to the refutation, on his so generally accepted authority, of a common enough idea that noxious vapors passed up a chimney become innocuous. If I am not mistaken, short of whatever heat may have been in the chimney in question, this is precisely—the heat element, however, being altogether omitted—the theory of sewer ventilation by shafts!

On the occasion in question, however, I did not refer to ordinary fires, but to distillation and destructive combustion in red-hot closed retorts, and as I do not pretend to any scientific knowledge whatever upon the subject personally, I had simply confined myself to the chemical papers of the discoverer, and to the medical testimony of the health officer of Glasgow, as well as that of Dr. Andrew Feargus of the same city. I had known their opinion, given for the express purpose, to have been acted upon, and I take from the substance of letters expressly addressed to me on the subject by Dr. Russell the health officer, and by Dr. Andrew Feargus, the following unquali-

fied statements. Dr. Andrew Feargus says that the water-carriage system causes decomposition injurious to health, and very much increases the power of infectious matter to spread disease; also that, on the contrary, a chemical treatment, which he goes on to specify, and which consists simply in the application of charcoal, renders decomposition impossible, and meets every sanitary requirement. Dr. Russell says, "No hospital should discharge into public sewers," and adds with reference to the system I had seen, approved and recommended with particular reference to hospitals, that "it would answer perfectly, and attain the end of destroying the possibility of infection spreading from hospitals in the manner referred to."

The testimony of Mr. Simon as to the preventibility of many forms of disease is probably known to you all. He says:—

"It cannot be too distinctly understood that the person who contracts cholera in this country is, *ipso facto*, demonstrated with almost absolute certainty to have been exposed to . . . pollution . . . that the diffusion of cholera depends entirely . . . upon the numberless filthy facilities for fouling earth, and air and water, and for the infection of man. Sodden earth, reeking air, tainted water—these are for us the causes of cholera."

Cholera, however, is but one of many kindred; diptheria has not been epidemic till water was misapplied. Dr. Richardson's table of diseases from organic poisons adds thirteen others to the list. "It is to be hoped," says Mr. Simon, "this sort of thing will come to an end, that so much preventible death-rate will not always be accepted as fate, that for a population to be poisoned by its own art will be deemed ignominious and intolerable."

There only remains for me now to submit, for your adoption, as I trust, the principles of the following programme. I have intentionally drawn them comprehensively, so as to embody, as far as possible, principles only, leaving the detail of arrangement to be framed by a subcommittee which might be appointed to consider the matter, and to final adoption by the Society on the recommendation of its Council. The following is the programme I would suggest:—

1. The Society of Arts, impressed with the importance of the subject, forms a Section for the special purpose of the promotion of Sanitary Science.

2. It invites, through the Lords-Lieutenant of Counties, the formation of Auxiliary Associations in every county and shire of the United Kingdom for the same object.

3. It proposes that these be incorporated with the Society.

4. It recommends the formation of Societies for the same purpose in all the several parts of the empire.

5. It will communicate with this object with her Majesty's Secretary of State for the Colonies, with a view of bringing the efforts of this Society before the several parts of the empire, through such channels as in the several cases the governers, for the time being, of her Majesty's possessions may recommend.

6. It will make known its action in these respects to her Majesty's Secretary of State for Foreign Affairs, and will request suitable opportunities for inviting, through the channel of her Majesty's embasses, legations and consulates, the cooperation of such bodies as may be formed in foreign countries for similar purposes.

7. The primary object of the Section shall be to promote the improvement of all arrangements affecting the health of populations in respect of the vitiations of the atmosphere, of water supplies and of soil, which are incident to human and industrial existence.

8. The Section, in strict subjection to the Society, is purely honorary, and will not in any way connect itself with any commercial undertaking which may arise out of its action or otherwise.

9. It will, however, investigate proposals for attaining any of the objects for which it is constituted, whether or not they be brought forward commercially, and the Society will, upon the recommendation of the Section approved by the Council, grant gold and silver medals and diplomas of merit, signifying distinct approval, and these shall be accompanied by letters, specifying the grounds on which such recommendations and approval are are made and given.

10. The Council of the Society will apply to her Majesty's Commissioners for the Annual International Exhibition to make permanent the display of en-

gineering and architectural appliances which form a feature of this year's Exhibition.

11. They will also suggest to her Majesty's Commissioners for the Annual International Exhibition, that arrangements be made at once to enable exhibitors of such appliances this year to deliver such explanations of the several objects they exhibit as can be illustrated, with the advantage of the examples upon the ground.

12. They will suggest that this arrangement also be made permanent, in so far as they may relate to any novelty hereafter introduced, or of such features as it may be deemed desirable should be continuously made known.

I may briefly add that this draft and the outline of the scheme on which it is based have grown out of a special effort which I undertook individually, owing to the convictions of nearly my life-time, but which I deemed might at length be brought forward successfully when the attention of the country was momentarily concentrated upon the subject, owing to the terrible calamity with which it was threatened in the alarming illness of the heir apparent to the throne, the patron of art, and the approver of science—his Royal Highness the Pince of Wales. My energies were fully aroused, and my hesitation ended by a case of blood-poisoning which occurred at nearly the same time in the house of a very near relative of my own. My occupations interfered with my giving the indispensable attention to all the details necessary to working out this object independently; but, without troubling you with a list of names, or parading the influential encouragement by which I was supported, I will only say that I was fully convinced the proposal needed only to be suitably launched to meet with ensured success, and to attain in a great degree many of its objects. This step, I trust, has been gained to-night. At all events, my thanks are due, and are most heartily given to the Council of our Society, which has favored me with the present opportunity of bringing the subject under your consideration. I trust that you will join in these grateful acknowledgments, and approve the course which I propose. With regard to the imperial and foreign societies for similar objects to which I have referred, I will only add that I have reason to hope that the action the Society may take will be warmly supported and readily followed. In the Austrian empire more particularly my communications have met with a very kind and appreciative response. Not only has the nucleus of such an organization been already formed in that empire, but I actually received solicitations for permission, on the part of noblemen of distinction in that country, to become subscribers to any effort that might be organized here.

ON THE REMOVAL OF PHOSPHORUS AND SULPHUR.*

By GEORGE J. SNELUS, A.R.S.M., F.C.S., &c.

From "Engineering."

In speaking of the elimination of phosphorus during the puddling process, Dr. Percy, in his well known work on iron and steel, suggests that probably the phosphorus is eliminated by a process of liquation of fluid phosphide of iron from the pasty puddled ball. It may be worth while to quote the exact words, which are as follows: "Hence it is clear that by the direct action of the oxygen of the air upon molten pig iron, or even molten malleable iron, phosphorus is not eliminated in a sensible degree. To what, then, are we to ascribe the opposite result, which occurs in the process of puddling, in which there is powerful oxidising agency at work? Is it due to the effect of the liquid cinder? Although I cannot answer these questions in a satisfactory manner, yet I will venture upon a conjecture which I have long regarded as at least plausible.

"In the puddling furnace, the iron, as

* Paper read before the Iron and Steel Institute.

it 'comes to nature' separates in a semi-solid or somewhat pasty state, and is not molten, as it is in Bessemer's process, the temperature in the former case being much below that in the latter, in which intense heat is developed by the actual combustion of iron. Owing to the formation and persistency of these pasty lumps in the puddling furnace, which, when collected into balls, extend considerably above the surface of the bath of cinder, opportunity is afforded for the liquation or sweating out of any fusible compounds of iron such as phosphide; and it is in this manner that I conceive the removal of the phosphorus may in a great measure be effected."

Other writers upon the subject have adduced various causes to account for the remarkable fact that has been so long accepted as an axiom, that phosphorus could be almost perfectly removed by the puddling process, and not at all in the Bessemer converter; but I am not aware of any writer who, up to the time I made my statement at our London meeting last year, had publicly made known that the elimination of phosphorus in the pneumatic process depended entirely upon the basic nature of the slag.

I was led to doubt the correctness of Dr. Percy's hypothesis during my studies of the puddling and refinery processes at Dowlais, and when I came to study the reactions which occurred in the Danks process, the particulars of which were laid before this Institute, I was struck by the fact that a good deal of the phosphorus was eliminated while the iron was in a fluid state. I communicated this observation to Dr. Percy in March, 1872, and at the same time told him that I believed I had discovered why this occurred.

On comparing analyses of iron and slags at different stages during the processes of Welsh refining, puddling and Bessemer converting, I had previously noticed that while the phosphorus was comparatively easily removed in puddling, variable but generally small amounts were removed in refining, and none at all in converting, even if the iron were considerably overblown. A comparison of the slags from the three processes showed that while that from the puddling process was highly basic, that from the refinery was less basic, and that from the converter highly silicious. I also noticed that if a silicious fettling were used in the puddling furnace, or a very silicious pig iron used, the resulting metal was inferior; and hence while at Dowlais I strove to get a non-silicious pig for puddling and a non-silicious ore for fettling, and strongly reccommending for this purpose the Campanil ore from Somorostro. For similar reasons, when consulted some years ago by Mr. Jenkins, of Consett, about the difficulty then experienced in keeping his iron for plates free from red-shortness, I advised the use of a manganiferous ore moderately free from silica, which has, I believe, been used with success ever since.

It is a well-known fact that silica is a much stronger acid than phosphoric acid, and that it will displace the latter from combination at high temperatures, and sonsequently, unless the silica is thoroughly saturated, there will be no base for the phosphoric acid to combine with, and, therefore, in contact with metallic iron at high temperatures it will be reduced as fast as made, if, indeed, the phosphorus is oxidised at all under these conditions. These considerations led me to believe that phosphorus was removed in all proccesses just in proportion to the basic nature of the slag.

Having these facts before me, early in 1872 I considered how I could obtain invariably a highly basic slag in the Besemer converter, and I saw clearly that to obtain this result it was absolutely necessary to line the vessel with a basic or neutral material, and that no success could be expected so long as it was lined with ganister or any material containing a large proportion of silica. Carbon occurred to me as an infusible neutral substance, suitable in so far as it would not affect injuriously any lime or other oxide that might be added to form a basic slag, but it had other serious drawbacks Oxide of iron alone was too fusible to. stand the intense temperature of the blow. While thinking over these things, I was fortunate enough to make the observation that certain kinds of lime, when submitted to very intense heat, became indurated and converted into a form in which they were no longer acted upon by water. The observation was made while performing the experiment

recorded on page 249, vol. ii, of the *Institute Journal* for 1872. I had used a lime crucible for the experiment, which was to prove the power of free silicon to reduce oxide of iron. The crucible, embedded in lime. was submitted to the most intense heat of the Siemens steel-melting furnace at Dowlais. After the experiment, I noticed that the lime crucible had turned brown, and was no longer alkaline to the taste, while at the same time it was quite hard and stony in character, and absolutely unaffected by water. It at once occurred to me that lime in some form or other was the material I was in search of for lining the converter. The well-known fact that it does not do to burn lime at too high a temperature if it is to be caustic also occurred to me. This is known to be particularly the case with magnesian limestones. Hence I specified magnesian limestone particularly. From some experiments I made shortly afterwards, I found that it was possible to make bricks out of lime or limestone, provided that when lime was used it was crushed, quickly compressed, and fired before it had time to absorb moisture, but it required intense temperature to consolidate them, particularly if the lime was very pure, while a small quantity of oxide of iron or other fusible base facilitated this. If lime was used, it was necessary to be well burnt first, otherwise the material was not porous enough to allow the small quantity of carbonic acid to escape without breaking up the brick. Finding, however, that crushed limestone lent itself with considerable facility to such a purpose as lining a converter, being plastic and binding when moist—like clay, in fact—it occurred to me to use it in this form, and to fire the vessel at a very high temperature. This was comparatively easy to do in a small converter, but I found considerable difficulty when I came to try a 7-ton converter. There did not appear to be any difficulty in forming a fusible slag in a vessel lined with lime, since this base combines with an excess of oxide of iron to form a fusible ferrate of lime. It was only necessary if the waste of iron during the blow were insufficient to provide the necessary oxide of iron to form a fusible slag free from silica, to add oxide of iron specially, to which course there could be no objection, while the presence of the oxide of iron would doubtless be advantageous in other ways. At the same time, lime being a basic body, would not be so readily acted upon by the oxide of iron as a silicious body would be.

Following up these conclusions, I made several blows in a small (2 cwt.) movable converter soon after I went to the West Cumberland Works. The results of these experiments I have shown to many private friends, and I have now the pleasure to lay the details before the Iron and Steel Institute. The results with the small vessel were very satisfactory, as will be seen by reference to the analyses, and there can be no doubt as to the perfect elimination of phosphorus and sulphur when the slag is kept thoroughly basic. In order that there might be no doubt on the subject, I lined the same small vessel with the ganister in the usual way, when I found that the phosphorus refused to leave the metal, just as in the ordinary converter. I therefore patented the use of lime and limestone, magnesian or otherwise, in all the various forms in which it occurred to me that it was possible to use it, for "the lining of all furnaces in which metals or oxides are melted or operated upon while fluid," and I especially called attention to its use in the Bessemer converter, giving some details of the results of its use under these circumstances. I was perfectly well aware that limestone had been used years ago for lining the sides of puddling furnaces, but its use for this purpose was a very different thing, because the metal in the puddling furnace does not remain fluid at the end of the operation, for which reason it did not answer satisfactorily, and, as is well known, its use for this purpose is almost entirely discarded. I felt, however, that although I had found the solution of the problem, so long and anxiously looked for, that my plans required to be tried on a larger scale before they were fully laid before the public, not because there could be any question that the same chemical action that succeeded with one or two cwt. of metal was likely to be reversed when operating upon tons, but because I felt that there were many points of detail that would require to be worked out. These details have been filled up by subsequent work-

ers, but it is gratifying to me that the complete success now attained follows so closely some of the lines then laid down. I shall doubtless be asked why I did not at once follow up my discovery and put my plans into practice. My answer is that I had just taken the management of a concern, the interests of which were opposed to the solution of this problem, and therefore having secured the ground by patent, I was compelled to wait a more favorable opportunity for putting my plans into practice. There was also a good deal of scepticism amongst those interests as to the possibility of solving the problem, and none seemed to care to take it up. The details of my experiments are as follows:

In the first blow the converter was lined with ground limestone in a plastic state and fired at as high a temperature as possible for a considerable time. A small cupola was also lined in the same way. About 2 cwt. of Middlesbrough pig iron, in which the phosphorus was about 1½ per cent., was melted in the cupola, then run into the converter and blown in the usual way. The metal blew well and some spiegel was added at the end of the blow, which accounts for the rather hard carbon in the steel. On analysis the resulting steel was found to contain:

Iron, 97.842 per cent.	While the slag contained:
Carbon, .8	Silica (over) 6.0
Silicon, trace	Lime, not estimated
Sulphur, trace	Phosphoric acid, 3.2 per cent.
Phosphorus, .018	

In order to try whether sulphur could be removed at the same time, a second blow was made in the same apparatus, a quantity of melted mottled hematite being run into the converter, and lumps of grey Cleveland pig were thrown in with it to afford additional silicon. In order to trace the reactions which took place, samples of metal and slag were taken from the converter during the blow and submitted to careful analysis. The metal was scarcely fully blown, as will be seen from the final sample. Those taken during the blow yielded the following results:

(*See Table on following column.*)

It will be noticed that the silica in the slag is rather high in this case, and that the imperfectly blown sample No. 3 has not quite lost all its phosphorus and sulphur. The gradual diminution of these elements, however, is very conspicuous, and no doubt the fully-blown sample would have been sufficiently pure for ordinary purposes. The third blow was made from a mixture that was tolerably free from phosphorus and sulphur to begin with, in order to see whether it was possible to remove the last traces of these elements. This was a very perfect blow, lasting just fifteen minutes, and the exact times at which the samples were taken were noted. Further, in order to test the steel, some of the fully blown metal was melted in a crucible with 6 per cent. spiegel, and tested in various ways.

	First Sample of Metal.	Second Sample of Metal.	Third Sample of Metal.
Iron	..	98.29	
Carbon	3.0	1.25	.66
Silicon	..	.11	.042
Sulphur	.346	.274	.169
Phosphorus	.864	.314	.231
CORRESPONDING SLAGS.			
Fe_2O_3	12.1	..	7.1
FeO	23.8	41.9	39.2
Silica	44.8	25.0	19.2
Lime	14.8	..	24.0
Phosphoric acid	2.3	7.25	7.0
Sulphur	3	.352	.512

Third blow.—About 100 lbs. No. 2 Bessemer pig and 30 lb. grey Cleveland were melted together, and then blown in the same vessel.

(*See Table on following page.*)

The full blown metal was fluid, and set moderately sound. It forged and rolled well, and could be easily welded.

It stood hot and cold tests well.

A larger vessel was then prepared, and about one ton of highly manganiferous pig blown in it, containing about 0.3 per cent. phosphorus.

The blown metal contained:	And the slag contained:
Carbon . . .4	Iron . . . 36.7
Phosphorus . .006	Silica . . 12.65
	Lime . . . 28.2
	Phosphorus .519

The metal at the end of this blow was perfectly fluid, and the lining of the vessel remarkably good. The arrangements for tilting this vessel were imperfect, and the metal had to be allowed to set in the vessel.

The results were:

METALS.

	First Sample taken 2 Minutes from start.	Second Sample taken 5 to 6 Minutes from start.	Third Sample taken 10 to 12 Minutes from Start.	Fourth Sample, at End of Blow, taken 15 Minutes from Start.	After Melting, with 6 per cent Spiegel in Crucible.
Iron	..	..	..	..	98.44
Carbon	..	..	..	trace	1.30
Silicon	1.505	.046	.084 partly slag	.019	trace
Sulphur	.109	..	..	.030	.052*
Phosphorus	.327	.094	.076	.003	.052*

CORRESPONDING SLAGS.

Fe_2O_3	..	..	53.8	11.42
FeO	..	..	..	49.00
Silica	..	..	10.2	10.50
Lime	..	..	33.0	25.0
Phosphoric acid	..	..	2.88	2.05
Sulphur	..	..	..	.26

Lastly, a charge of 4 tons of metal was blown in a large 7-ton converter, but the lining had not been sufficiently heated, and the metal blew so cold that it could not be poured. As the vessel could not be spared for another blow, the experiments were suspended at this point. It appeared that the only serious difficulty was in getting the lining of the vessel, when put in the ordinary way, thoroughly burnt, and it occurred that a way out of the difficulty might be found in making bricks out of the material and burning them beforehand, but I did not go beyond satisfying myself that this was a perfectly feasible plan, and having to devote all my energies at this time to reorganizing the West Cumberland Works, further tests were put off. Although no experiments were made in the Siemens furnace, all chemists will agree that the reactions occurring in the Bessemer converter could be more easily obtained in the open-hearth furnace.

Some time afterwards Dr. Siemens was good enough to have his experimental rotator at Birmingham lined with limestone in a similar way, but it did not answer for two reasons, in my opinion: 1. The limestone was a peculiarly rich one, and would not bind well before burning. 2. The temperature that could be obtained in the rotator was not sufficiently high.

It is essential that the material used for lining the vessel, and for bottoms, should be as free from silica as possible, as some of this must go into the slag, and it appears to be necessary to have a large amount of base to saturate the silicic and phosphoric acids present in the lining material or bases added to form slag, or formed from the oxidation of the silicon and phosphorus in the iron. Unless this is done, the slag becomes too acid to allow the phosphorus to be eliminated. This is seen to some extent in blow No. 2. It is a question how much base is required, but I am inclined to believe that there must be sufficient to form the basic silicate and tribasic phosphate.

Mr. Roe, who is at present engineer to the Consett Works, was engineer at West Cumberland at the time these experiments were made, and assisted in performing them.

In conclusion, I have the pleasure to place before the meeting what I believe to be the first sample of Bessemer steel made entirely from Cleveland iron by one operation, in which the phosphorus has been reduced to a mere trace. A portion has been forged into a chisel, while a rough specimen of the same has still part of the lime lining attached to it. With the samples I have placed the original wrapper bearing the date when the sample was made, and also my notebook with the original entries of the details of the analysis.

* The increase in these elements was due to the spiegel added.

THE Commercial Department of the Swiss Federal Council are engaged in the preparation of a patent law and a law for the registration and protection of trade marks. Neither trade marks nor inventions have hitherto been the subject of legislation in Switzerland. It is, however, at last perceived that to retain native industry property in invention must be recognized.

THE INTER-OCEANIC CANAL.

From "The Engineer."

FEW great engineering projects have alternately attracted so much interested attention and suffered periods of slumber so unnoticed, as those which have since many years proposed to separate the two Americas by a ship canal. The vastness of the work to be done, and of its cost, has apparently produced an inward conviction upon men who in turns approached, examined and found it beset with practical difficulties, that it would be impossible to construct the canal as a commercial speculation, and that, as an international work, the chances of securing favorable attention were too small to make it advisable for any man to spend his time in the matter, in the hope of getting a certain line adopted. Different men, however, some of whom have secured concessions for the construction of a canal, have looked upon the scheme as hopeful, and, as announced in our columns recently, an International Congress has been called together to consider the relative practicability of all the different schemes which have been from time to time put forward. The meetings of the Congress have been held under the presidency of M. Ferdinand de Lesseps, in apartments of the Paris Geographical Society. The deliberations of the Congress extended over rather more than a fortnight, meetings being held first to discuss the general question, and afterwards to receive and vote upon the reports of the different committees appointed to examine the relative superiority, from all points of view, of the seven different projects which had been presented. The report of the technical committee was the last presented, and this, it need hardly be said, was the most important of the whole, as affording the means of judging of the propriety of any longer considering the question as a commercial speculation. Of course the several schemes were accompanied by statements of probable cost and revenue by those who had presented them, but it was, nevertheless, necessary that these vital questions should be examined by a committee calculated to "take out the quantities" for each scheme with equal impartiality.

Seven schemes were brought before the Congress, and sites for the canal, as proposed by six of these, lie within the United States of Colombia, while that of the seventh lies partly in Nicaragua and partly in Costa Rica. The first of these would be 290 kilometers in length, though only fifty of these would be entirely new canal, 240 being of the rivers Atrato, Napipi and Dognado. It would commence at the Gulf of Uraba on the Atlantic and end in the Chirri-Chirri Creek on the Pacific. Twenty-two locks were required by this route. The second scheme was for a canal from the Gulf of Uraba, a short distance along the Atrato and the Bay of San Miguel and Darien Harbor. The route of another scheme is partly along the line of the preceding, into the Darien Harbor, and thence through 17 kilometers of tunnel to Acanta in the Bay of Uraba. A fourth canal would connect the Bay of San Blas and Panama Gulf, while the fifth and sixth schemes, which are coincident for the greater part of their length, are in the Panama State, and connect Colon or Aspinwall on the Atlantic side and with the little Bay, situated at the mouth of the river Rio Grande and of Panama respectively. These two proposed sites for the canal occupied the most attention, as being on the whole more free from objections than any of the others. In order that the financial and technical engineering and maritime questions should all be considered in a systematic manner, the Congress was at an early stage divided into five committees, whose reports were discussed at a general meeting. The first committee devoted itself to the statistical part of the undertaking, such as the probable amount of traffic and the share which each country will take in it; the second considered the economical and commercial aspect of it; the third investigated the question of navigation and shipping; the fourth, technical questions, such as the cost of making and keeping up the canal; and the labors of the fifth

were confined to the subject of ways and means.

Before all these committees had sent in their reports the Congress had become convinced that to be commercially successful the canal must be free from the obstruction of locks and sluices. To meet this condition, the choice of routes was confined to schemes referred to as the third, fourth and sixth, the seventh, or Nicaragua route, being out of consideration, owing to the very manifest disadvantages of absence of harbors, insalubrity of the Atlantic slope of the country, the great length and the unstable character of the countries. Of these three schemes, the first mentioned required a canal of 125 kilometers, of which 51 would be formed of the rivers already mentioned. This route, however, required a tunnel of 17 kilometers, which, in spite of the advantages in respect of ports and salubrity of climate which the route offers, was sufficient to condemn the scheme. The fourth scheme, or San Blas Bay route, was out of court for the same reason. We now come, then, to the route which has been selected by the Congress, namely, that which connects the Bay of Lima and the Gulf of Panama. This route is nearly parallel with the railway which connects Panama and Colon, or Aspinwall, and is 75 kilometers, or about forty-six miles in length. Of this, 6 kilometers, or $3\frac{3}{4}$ miles, are of tunnel in tenacious rocks, and though the rivers Chagres and Rio Grande will be utilized, there will be 47 million cubic meters of material to remove to make the canal. The country through which it passes is already well populated, and the existence of the railway will be a source of the greatest practical convenience in the construction of the work, the removal of the material, and the distribution of supplies. That the country is well inhabited is looked upon as a great advantage, and the existence of the two towns at the harbors is of course much in favor of this route. The short length of the tunnel and slight elevation of the land in general are favorable to the construction of the canal in about six years. Among the objections to this route are the great heat and the well-known deadly nature of the climate at some parts of the year. During some months the climate is good enough, but that it is not so throughout the year became only too evident during the construction of the Panama Railway, parts of which it has been said might almost have been ballasted with bones of the dead. The hardness of the rocks to be dealt with is a matter of some concern, but possibly the arrangement of an indemnity for the railway company, whose traffic would of course be seriously affected, would be a matter more costly to achieve. To some of the routes the objection of liability to volcanic destruction was urged, and the long absence of seismic or volcanic action was placed as a note of credit to the Panama route. That a considerable period has elapsed since the occurrence of any destructive earthquake near Panama is no doubt true, but that this immunity should remain permanent while evidences of volcanic and earthquake action of no slight character are so common a few score miles therefrom, does not seem sufficiently established to make security or insecurity of the work either certain or disproved. The estimated cost of the construction of the canal, which, with every desire on the part of the committee to be accurate, can only be taken as a very rough approximation, has been given at 1,070,000,000f., which, with 130,000,000f. capitalized at 5 per cent. to provide for annual cost of repairs and maintenance generally, makes the total estimate 1,200,000,000f., or nearly £48,000,000 sterling. This scheme then to be worth the attention of capitalists must return a clear profit of about £2,000,000 per year. It was pointed out in one of the reports that in order to make sure of the success of the canal, it would be necessary that 6,000,000 tons of shipping should pass through it yearly, or a daily passage of eight ships of 2,050 tons each. It was calculated that after the harvest sixteen vessels would pass through the canal daily. From this it will be seen that it is proposed to charge about 6*s.* 8*d.* per tons for passage through this canal. Fifty ships should, it is urged, be able to pass through per day, but as has been stated it is only estimated that sixteen ships will pass through the canal after the American harvests when the traffic would be greatest, so that even at this busiest time each vessel would have to be over 1,000 tons burden. Now supposing that the cost exceeded

£48,000,000, as we may say it certainly would, and probably by several millions, it will be seen that the canal, to pay anything at all, must induce a traffic which does not now exist, and then must make it all pay a toll of 6*s.* 8*d.* per ton. As a commercial speculation the scheme does not promise well, and though on possession of the final reports of the committee, a somewhat better case may appear to have been made out, the one fact that at least £48,000,000 sterling are required is sufficient to frighten capitalits.

The share of the most venturesome financier would be but as a drop in the ocean in such a vast sum, and the very uncertainties connected with it are sufficient to deter the private capitalist.

The Congress at Paris has cleared away the cobwebs which many proposals had woven over the project of cutting through the Isthmus, and has pronounced a carefully sought opinion as to the best route for a canal, and of the practicability of the work. That the work will never be constructed by private enterprise, however, may be looked upon as certain, and it seems questionable whether the few countries that are directly interested in the work in a commercial sense will not think that £48,000,000 is too dear a price to pay for a thing which might not be an unmixed blessing.

A NEW REVERSING LINK FOR STEAM ENGINES.

By C. A. SMITH, M. E.

Written for Van Nostrand's Magazine.

There are various devices now in use for reversing the motion of engines: such as locomotive engines, marine engines, etc. In nearly all these devices the valve is operated by a "link" connecting the ends of the eccentric rods, whose eccentrics are placed side by side on the engine shaft—or axle, in case of the locomotive. Some of these devices have been classified by Auchincloss* as "the stationary link motion," "the Allan link motion," "the shifting link motion," etc. All these link motions are doubly valuable since they not only enable the engineman to reverse the motion of the engine, but also to "cut off" the steam at any part of the stroke, according to the power required, being thus a "a variable cut off" as well as a reverser. Although the link motions possess these good qualities, they are not free from objections. Some are too complicated, and some have other objections. One of the most simple, and the one almost exclusively used on locomotives of this country, is the shifting link motion. This still exists substantially in its original form as first used on the locomotives of Robert Stevenson, over a quarter of a century ago. It generally gives good results if properly designed, and yet there may be room for improvements.

Some of the objections to ordinary link motions may be briefly stated as follows: In the link, the working surfaces of both link and link-block must be perfectly true, and of the exact required curvature in order to fit nicely and work properly. This requires special machinery—generally an attachment to a planer—and skilled labor of a high order in fitting up the parts, especially when the working surfaces are case-hardened, as they always should be.

When the link and link-block become worn so as to work loosely, there will be lost motion of the valve, and this has always been an objectionable feature in link motions. To take up this lost motion requires, in some cases, a new link-block, and in all cases a complete refitting of the link.

In designing link motions, equalization must often be sacrificed for the purpose of reducing the slip and the consequent wear of the link-block, since this is the inevitable cause of the lost motion referred to.

Figs. 1 and 2 illustrate a form of link† by the use of which, it is believed, all

* Auchincloss' "Link Motion," page 91.

† Patented May 14, 1878.

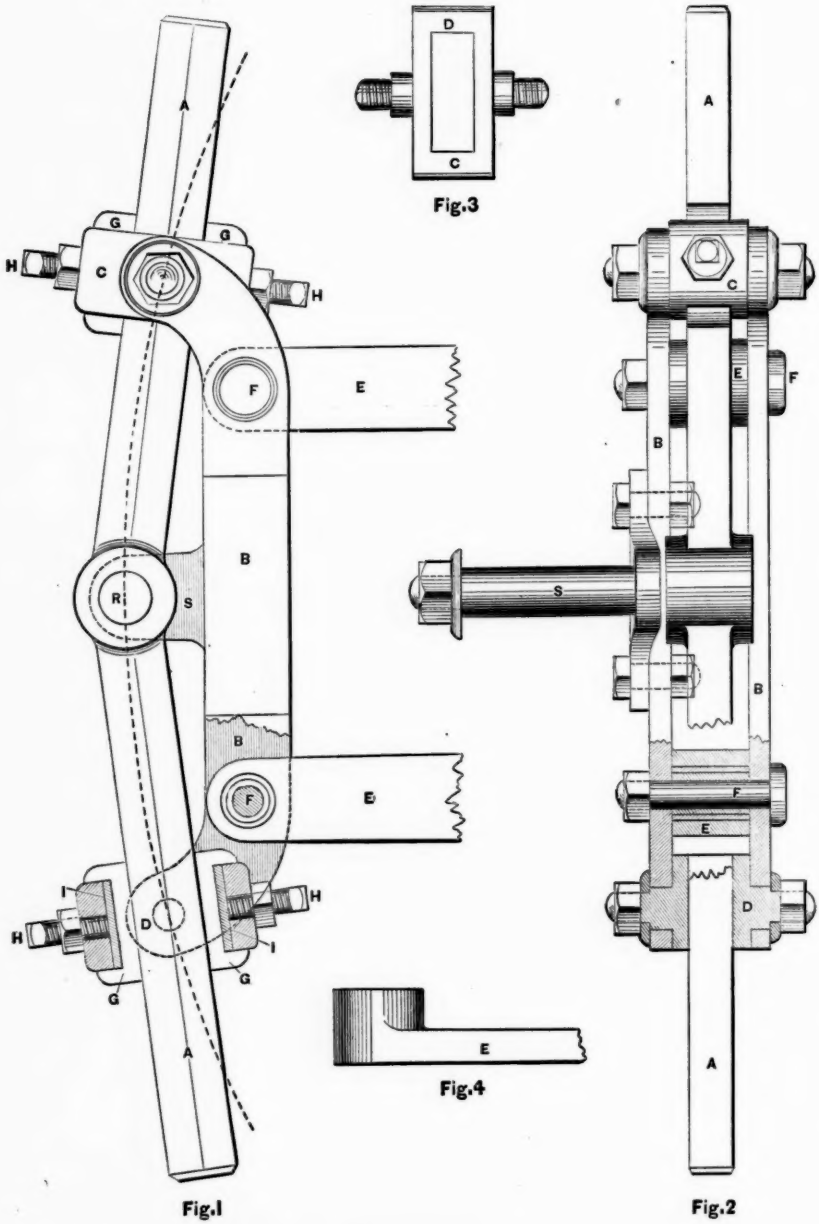
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Fig.2

these difficulties, among others, will be entirely overcome. Fig. 1 being a side view of the link, and Fig. 2 an edge view. The engravings will explain themselves as to the detailed construction of the link. The principle upon which this link is based, may be stated as follows: If two right lines, CR and DR, Fig. 6, intersect, making a constant angle with each other, their point of intersection, R, will describe a circle when the lines are respectively moved along the fixed points C and D.* This principle is made use of to guide the rocker pin in a circular arc (the link-arc) instead of the link-block sliding in a circular slot as in the ordinary link. This is accomplished, as seen in Fig. 1, by the angular bar A, whose center lines AR correspond to those of Fig. 6. The rocker-arm, or valve stem, is attached to this bar at its middle point, R. The bar A—which we may conveniently call the *link-angle*—passes through the pockets C and D (shown in Fig. 3) which are attached to the link proper, BB, by means of swivel joints as seen in Fig. 2. Now it can be seen that when the link is shifted, the center points C and D will move along the middle of the link-angle and the center

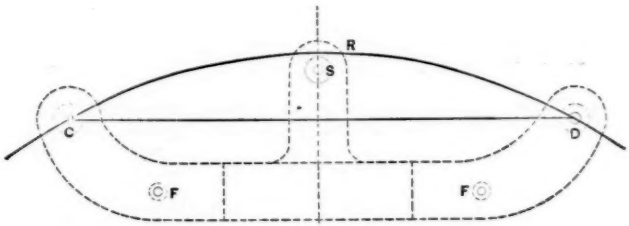

Fig. 5

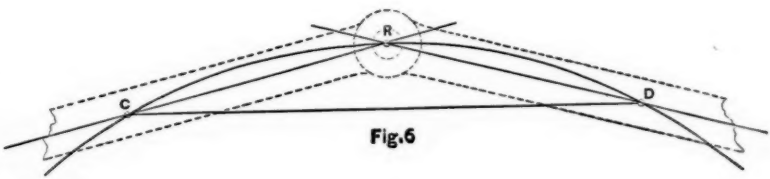

Fig. 6

of the rocker pin, R, will travel, relatively, on the circular arc CRD.

To keep the distance of the eccentric pins, F, from the link-arc always the same, a very small groove is cut, with a "V" pointed tool along the middle of each arm of the link-angle, as shown by the fine lines in Fig. 1. Similar marks are made on the edges, opposite the centers, of the pockets C and D. This being properly done, the set screws, H, can be so adjusted that the centre line of the link-angle will cover the marks on the pockets.

In designing this link we may proceed as follows: With the given radius of the link describe an indefinite arc, as CRD, Fig 5. This will be the link-arc. Upon this arc locate the points C and D, making the chord CD sufficiently long to give the required travel to the rocker-pin upon this arc without striking between the boss in the middle of the link-angle and the gibs, G. Then locate the centers, F, F, for the eccentric pins the required distance from the link arc, CRD, as in the ordinary link. Also the point of suspension, S, is located in the middle of the link, a little back of the link-arc as usual. Having located these five points, the link may be delineated about them as shown by the dotted lines.

To determine the angle between the two branches or arms of the link-angle, describe an indefinite arc, CRD, Fig. 6, with the desired radius of the links, as in Fig. 5. Upon this arc locate the points C and D, making the chord CD equal to

* Davies' Legendre, page 79; Rankine's Machinery and Millwork, pages 261, 262.

the distance CD of Fig. 5. Assume a third point, as R, upon this arc and draw the straight lines CR and RD through these points, then will CRD be the angle sought. The link-angle may now be drawn, as shown by the dotted lines, its edges being drawn parallel to the lines CR and RD, and the center of the hole for the rocker-pin should coincide with the intersection R of these lines.

When a center cannot be conveniently located from which to describe the arc CRD, the angle CRD may be determined as follows: Assume a center, O, upon a straight line RD, Fig. 7, and with any convenient radius, as RO, draw a semi-circle, RAB, cutting the line RD at R and B. With B as a center and radius

$$BA = \frac{RO \times CD^*}{r}$$

describe an arc cutting the semi-circle at A. Through A and R draw the line ARC, then will CRD be the desired angle, about which the link-angle may be delineated, as in Fig. 6. It will, of course, be understood that the greater the radius RO is assumed, the more accurate the angle can be laid off.

At this point it will be evident to the

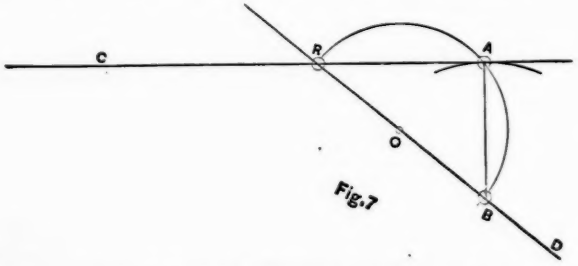

Fig. 7

reader that the *cinematique* of this link is the same as that of the ordinary one, since the relative positions of the link-arc and the points F, F and S are the same in both cases.

In applying this device to the shifting link motion, it will require no more space to work in than the ordinary link, since the link-angle A is fixed in position to the rocker arm while the link B is shifted. In the stationary link motion, however, the link B is fixed, while the link-angle A would have to be shifted to reverse the engine. In this link motion, therefore, this would require more space to work in than the ordinary link, and hence it is more especially adapted to the *shifting link motion.*

Among the advantages this link possesses over the ordinary form now in general use, are the following: The working or sliding surfaces are straight, requiring no special machine for planing a circle as is the case with the ordinary link. The planer work can be done on an ordinary planer, *without* attachments, and it requires less time and labor to fit up than the ordinary link. There need be no lost motion, such as results from wear between link and link-block of the ordinary link, for if the link-angle A, from wear and friction, becomes loose in the pockets C and D, the defect may be immediately remedied by simply tightening the set screws, H, and setting the gibs, G, closer to the bar. There will be less work on the ends of the eccentric rods, since this link requires simply a boss on the end, as shown in Fig. 4, instead of a fork as with the ordinary link. In using this link on any engine the link motion may be equalized as near as possible without regard to the slip, because the lost motion, resulting from this, can be readily taken up. Hence, the problem of the link motion can be considerably simplified by eliminating the slip from its solution.

The details of construction may be varied in many ways without affecting the fundamental principle, but, all things considered, the design given in Figs. 1 and 2 is perhaps the most economical form that can be adopted.

A link constructed on this principle is especially adapted to link-motion models when the link-angle, A, is made with a joint in the middle so it can be set at any

* "CD" is the distance CD on the link, Figs. 1 and 5, and r is the desired "radius of the link."

angle to vary the radius of the link-arc. Master mechanics using models to work out their link motions are obliged to make a new link every time the link-radius must be changed. With this link, properly constructed and graduated, the radius can be varied by simply changing the angle CRD, which is the work of but a moment.

The formula

$$\text{sin. CRD} = \frac{\text{CD}}{2r}$$

gives the value of the sine of the angle for any link-radius r.

THE CONSTRUCTION OF HEAVY ORDNANCE.

By JAMES ATKINSON LONGRIDGE, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Mr. Longridge desired to add a few explanations. All the formulæ had been reduced to the same notation as his own, which consequently differed somewhat from that given in the text book on Ordnance, issued under the authority of the Secretary of State for War. Professor Barlow's formula was correctly stated in the text book, but Dr. Hart's incorrectly. The plus sign ought to have been put for the minus sign in the latter. Not only had the formula been printed wrongly, but the calculation based upon it was wrong. But the formulæ did not really solve the question. The point to be ascertained was, if there was a cylinder with a given pressure inside, and another pressure outside, what would be the tensile strain induced in the material? The formulæ quoted in the text book did not contain the pressures. The proper formula was given in the Paper: "f_n" represented the internal and "f_{n+1}" was the external pressure; consequently for every ring there was a pair of equations. Then there was another equation, which depended on the modulus of elasticity, which was where the two rings were in contact; so that in the case of a gun with three rings it was necessary to determine three inside and three outside tensions and two normal pressures—eight unknown quantities, and for this there were eight equations. The question was therefore as solvable as any question connected with the building up of a girder. If the modulus of elasticity and the dimensions were known, there was no more difficulty in solving the exact strain at any point in a compound gun built up with rings, than in solving the strain on the upper or lower web of a girder. Unless guns were built on that scientific principle, the same trouble would be experienced as if an engineer were to build any large iron bridge without knowing the formula representing the strains. True, by giving it an enormous strength a bridge might be obtained that would stand a long time; but if the limits of elasticity were passed in any element, the bridge would assuredly, sooner or later, fail. That system had been acted upon in the manufacture of the Woolwich guns. It would be seen by his calculations that at certain portions of the structure, where the strain was beyond the limit of elasticity, a permanent set must occur, and that it must give way at every successive firing until the tube cracked. He desired to take that opportunity of expressing his conviction that the late accident in the "Thunderer" had nothing to do with the Woolwich system. He was certain that Woolwich guns would not burst explosively under any ordinary circumstances. He had an opinion as to what was the cause of the accident, which he could not then enter into; but he was certain that the explosion was not due simply to the structure of the gun. The gun exhibited had been made on the system which he had long advocated. It had been fired in the autumn of 1860, after his first Paper was read, at Southport, and he had since then made experiments

with it at the Mauritius. It had been fired many times with heavy charges; it had succeeded perfectly, and there had never been the slightest accident with it. In firing there was no possibility of the slightest escape of gas. He hoped that in the course of the discussion the principle would be considered more than it had been on the previous occasion, when many interesting matters were discussed, but the principle he had brought forward had scarcely been touched.

Colonel J. D. SHAKESPEAR, said there were two essential points to be considered, to which all others were secondary in importance. The first point was the exact part where there was the greatest strain on the gun. That point had not hitherto been settled, and he was somewhat surprised to hear Mr. Longridge state that there was an easy formula for calculating it, as it did not appear to him that use had been made of it. Several years ago he had seen it carried out in a rough and ready but practical way. Holes were bored along the first and second reinforces. They were countersunk just sufficiently to steady a 32-lb. shot. The holes were about as large as a good-sized rent. The gun was fired, and it was calculated what height the shot was thrown into the air. By that experiment it was ascertained that the greatest strain on a gun was a few inches in front of the projectile. That seemed to be in accordance with common sense, because the first ounce of a 100-lb. charge would probably move the whole. The greatest strain would take place when the greatest amount of the charge exploded, and that would be some little distance up the bore. He had observed in his experience, that guns almost invariably burst near the trunnions. If the force of a fowling-piece bursting was in the breech, it would knock a man down; but many persons had a gun burst in their hands, who were absolutely unconscious that such an explosion had taken place; hence it was evident that the violence of the charge was not in the breech. There could be no doubt the principle of placing nearly all the metal of a gun in the breech was a wrong one. The right principle was to place the greatest amount of metal where the strain was—somewhere in advance of the projectile; and he believed that was the intention at Woolwich. With regard to the "Thunderer," it appeared from the drawings in the *Illustrated London News*, that the tube had broken away immediately in front of the second reinforce, and that was about the point where the end of the projectile would be. With regard to the skrinking of the metal, that was an exceedingly delicate matter. Round a cold tube a bar of iron, almost at white heat, was entwined; it became chilled in contact with the cold iron, and the surface set; the outer surface became set sooner than the inner metal; the two surfaces were hard set, and between them was a soft, spongy metal. It was not possible, therefore, on the principle of shrinking, to get the whole gun homogeneous. With regard to the use of hot water for chilling, it should be remembered that water at 212° was cold in comparison with the heat of the gun. That would always be one of the great difficulties in shrinking guns; yet, considering the number of guns constructed in that way, and the small number of failures, he could not condemn the system. He regretted the absence of Sir William Palliser, who disapproved of the Woolwich plan, but might not approve of this; he had a method of his own, which had been largely carried out in America. He believed there was a large field still open for the construction of heavy artillery. He did not know how the desired result was to be accomplished unless by shrinking, and making the shrinking perfect. In the case of the gun represented in Fig. 9, it appeared that all the metal was in the breech. That gun, he thought, would burst about the letter x, which would be rather in front of the projectile.

Admiral Sir COOPER KEY desired to make a few remarks, as one who had had some experience in the working of heavy guns at sea. He would not follow the Author through his mathematical calculations, but he wished to ask a few questions. If it were possible to calculate the exact tension that the wire required, and if it were possible practically to apply it accurately round a tube, he believed it would make the strongest gun to resist circumferential strain that could be made with equal weight. It certainly carried out the coil principle of making guns, which he considered the

best, to an extreme. The smaller the coils, and the more of them, the more reliable the gun would be. There was a point, however, about the proposed gun that he did not like. The Author appeared to think that the longitudinal strength was sufficiently provided for. Admiral Sir Cooper Key imagined that in a 10-inch gun the pressure on the bottom of the bore, in a direction contrary to the path of the shot, would be from 1,200 to 1,500 tons; and it appeared from Fig. 9, that it was intended to take the whole of that strain on four or six bolts. He could not think that would in any way resist the strain. He objected to the use of cast iron, either in the exterior of the gun or for the inner tube. Although the Author had proved that in a 3-inch gun the concussion was not sufficient to damage a tube only half an inch in diameter, he felt satisfied that the enormously increased concussion of a heavy gun would be more than any cast iron would stand. He was glad the Author had turned his attention to the breech loading of heavy guns; it was one of the chief requirements of the present day, as guns of great power must be of great length. He should be happy if manufacturers would turn their attention to the best means of providing a good system of breech loading; but he could not say that he liked the Author's plan. The breech piece was only supported by two bars on the small trunnions; and he could not think that such bars, of any size that they could be practically made, would be sufficient to resist the enormous strain at the bottom of the bore in the line of the axis of the gun. In breech loading the convenience and efficiency of the mode of entering the projectile and cartridge were of great importance. A small gun like that exhibited would tip up if the trunnions were properly lubricated, and if just sufficient preponderance were made on the chase end; and the weight of the shot would bring it down again. But in the case of a heavy gun, where the weight on the trunnions would be enormous and the friction necessarily be very great, the shot, placed so close to the trunnions as it must be, could not bring down the heavy breech.

Colonel Shakespear said that the chief difficulty experienced from breech loading on board ship, as well as in casemates, was in connection with the withdrawing of the breech piece, as the men were stifled with the smoke.

General Sir H. Lefroy remarked that no one, at Woolwich or elsewhere, could have any other than a grateful feeling to the Author of the Paper for the ability and mathematical skill he had brought to the investigation of the problems with reference to the expansion of rings or laminae of metal under tension or compression. It had, however, been well remarked, that the British empire was governed not by logic but by Parliament; and it was equally true that mechanics were governed not by formulæ but by facts. He was far from saying that there was not a substratum of facts in what was contained in the Paper with reference to various moduli. He observed, however, that, in one instance, the Author had taken 24,000 for his modulus, and, shortly after, 13,000.

Mr. Longridge, interposing, remarked that he had thought it desirable to take a great range, his object in the case quoted being simply to show the effect of varying the modulus.

Sir H. Lefroy continued, that the published works of the officers connected with the Royal Gun Factories were of course fair subjects of criticism, and if they had fallen into mistakes, it was desirable that these should be pointed out. But when it was stated that the rules adopted in those factories and elsewhere were empirical, one was tempted to ask, Why were they empirical? Surely it was not because there was not sufficient mathematical ability at Elswick or at Woolwich to comprehend and apply a formula! The truth was, that experience taught much more than formulæ; and it had shown that the details as to the shrinkage of metals, which were subject to a wide range of variation, were not worth pursuing into a maze of figures. He confessed that he was surprised when he first heard of the practical rule of shrinkage which Sir William Armstong had adopted; because in his simplicity he had imagined that every gun was worked out by formulæ. Since, however, Sir William, and others at the Gun Factories, had discarded formulæ, and had adopted practical rules of thumb, he thought it might be reasonably concluded that they

had some justification in doing so. In building a gun, as in building a bridge or any other structure, the engineer looked to three things: first, that the design should not be opened to any charge of structural defects; secondly, that the structure should have an excess of strength; and, thirdly, that it should be carried out as cheaply as possible. It was the latter consideration that had led to the preference at the Gun Factories at Woolwich to a small number of thick coils over a large number of thin coils, which were still preferred at Elswick. The Author, in contrasting the cost of manufacture under the two systems, had charged himself with the exact quantity of metal turned out in the finished 45-ton gun, and he had also put the cost of the metal pure and simple, without anything for the cost of manufacture. There were different kinds of material in the gun—steel wire, solid steel, cast iron and wrought iron; materials acting differently with different strains, and under different temperatures, and they were all combined in an artificial manner, involving conditions of which they had no experience. How a great coil of steel wire would behave in the tight embrace of a hot iron jacket he did not know. It did not appear to him that the conditions of strength in the proposed structure were such as to warrant the Government in abandoning a well proved system of construction.

Mr. Longridge replied, that there was not the slightest shrinkage; the outer casing was not cast round the inner tube, but was a separate casting, into which the tube was apparently slipped, and it acted only as a sheath, and had nothing to do with the strength of the gun. General Lefroy had fallen into an entire misconception as to the way in which his guns were made, and it would perhaps save time if he briefly explained the method. In the first place he made a tube comparatively thin. For a gun of 20 inches bore the tube would probably be 3 inches or 3½ inches thick. It might either be of cast iron or of steel; that, however, was a question into which he would not enter, simply remarking that his opinion was that cast iron might form the best material. Upon the tube he rolled a coil of wire, $\frac{1}{16}$ inch in diameter, from one end to the other, with a tension varying according to the distance from the breech. It was then rolled back, with another tension gradually increasing. Thus he went backwards and forwards till he obtained the thickness shown at the breech end for a certain distance, gradually decreasing towards the muzzle; the result being that, when the gun was subjected to internal pressure from powder gas, every wire, from the innermost to the outermost, was strained to the same degree. The inner tube had nothing to do with the strength of the gun; it simply transferred the pressure to the wire; and he could so arrange it that the inner tube, 3 inches thick, with a strain of 26 tons or 27 tons per square inch inside, should have no tension at all. That was a matter which must be taken upon trust, as he could not then go through the calculations to prove it. Having completed what was really the essential part of the gun, he had a casting divided through at the point shown in Fig. 9, and held to the breech casting by the bolts. This was intended to preserve the wire from shot or any external injury. The other part, which constituted the main weight of the gun, was like an anvil to receive the blow. The gas check was fitted in the tube; and the bolts held together the two portions of the gun, having nothing to do with the recoil. The recoil was upon the trunnions, and the gun rolled up an inclined plane. In the case of a gun like the one exhibited, the whole recoil was taken up by the side links. At that part the gun had no longitudinal tension. The great principle which he maintained in building breech-loading guns was, that the material that resisted the circumferential strain should have nothing to do with the longitudinal strain. There was a direct strain upon the side rods, which manifestly might be made of any necessary strength. There was no limit to the size which a gun might be made in that way. There was no compound circumferential and longitudinal strain, and no cutting away of the breech. The cast iron parts were simply protections, and means of getting weight. Weight was necessary in the case of a large gun on account of the recoil; but he could get weight, employing material at £5 or £6 a ton, that would answer every purpose, and serve

as a protection. With regard to formulæ, it should be remembered that if a formula was right it could not differ from facts. All he maintained was, that there should be some formulæ to guide the manufacturers of guns. The Board of Trade had laid down certain limits in the case of bridge building within which the material should be strained; and, in like manner, he thought that in building a gun there should be certain limits beyond which no portion of the gun should be strained; and to ascertain those limits formulæ were necessary. He believed that the formulæ he had given represented, not perhaps with perfect accuracy, but very approximately, the conditions of a gun, and enabled the maker to determine the strain. He did not say, as General Lefroy seemed to imply, that there were no mathematicians at Woolwich; but he believed they had not applied mathematics to the problem in question.

Dr. SIEMENS said, the Author complained, and with some reason, that on the occasion of the reading of his previous Paper, nineteen years ago, the discussion had wandered away a good deal from the lines laid down by him. The present Paper presented such clear lines that it was not difficult to keep within them, and he would endeavor to do so. The Paper might be divided into two parts. The first was the critical, and, he might say, destructive part, because the criticisms were such that, if completely borne out, one would almost be afraid to contemplate the result of a general war. The second part was suggestive or constructive. With reference to the criticisms on the Woolwich system, he was ready to admit that the Author was very clear in his mathematical deductions. He had pointed out some error in one of the formulæ used at Woolwich. Passing that over, he thought that the formulæ which were used—those of Professor Barlow and Dr. Hart—were not erroneous in themselves; but, as the Author had shown, were insufficient to meet the case of the strain in every portion of the structure. The author had from that drawn the conclusion, that in not following out a formula, such as he had presented, the authorities had gone altogether wrong, that the strains by shrinkage which were applied at Woolwich were totally opposed to theory, and that much better results might be obtained in strictly following out the dictates of mathematical reasoning. Mathematics, however good a handmaiden, might be a very dangerous master; and he thought he could show where the Author had made mathematics his master. He had given his idea of a gun constructed on the Woolwich principle—a nine inch gun. There was a steel tube of 8 inches thickness of side, around which he shrank iron to a thickness of 28 inches, with a shrinkage exceeding that usually given at Woolwich, namely, of 1 in 1,000. Dr. Siemens knew from experiments of his own that a shrinkage of 1 in 1,000 meant a strain of 12 tons per square inch. If the shrinkage was 1 in 1,000 upon the inner diameter, there would be a strain of 12 tons upon the inner surface of the coil. That in itself presented a very good strain for a material such as iron. If, however, round a tube of 8 inches inside, 28 inches of iron were shrunk in such a way as to put the whole into tensions varying in elastic strain inversely as the diameter, what would be the result? That the strain on 28 inches would be opposed by the 8 inches; that the area of the outer metal would be resisted by the lesser area forming the side of the lining. The Author, in all his calculations, spoke only of what would take place when the powder pressure of 24 tons to the inch was applied; he did not say what would be the case when the shrinkage pressure was applied in the first instance. That shrinkage pressure would undoubtedly have the effect of crushing or deforming the tube permanently. The tube would be no more able to resist such an amount of shrinkage pressure than if it did not exist at all; and therefore such shrinkage as was contemplated by Mr. Longridge could not practically be applied. The Author had criticised rather severely some remarks in the Woolwich reports. Referring to the statement that in "guns of present construction the heavy breech coil compresses the steel barrel to such an extent that the latter becomes in some instances as much as $\frac{1}{100}$th of an inch smaller in diameter during the process of shrinking," the Author had remarked, "It is difficult to imagine a more complete confusion of ideas than that which

pervades this sentence." Dr. Siemens believed the statements in the official report were perfectly consistent, not with mathematical, but with engineering or physical facts. It would, he maintained, be impossible to give such shrinkage to the mass of iron surrounding the inner tube as would leave that mass of metal, under a sufficient tension, to take its full proportion of work when an inner pressure was applied equal to 24 tons to the square inch. But suppose that the inner tube could be made strong enough to resist such a pressure, the use of the gun had then to be considered, and the result of the first few rounds. Shrinkage would then again take place, but upon the wrong side of the gun; that was to say, expansion by heat would occur inside the gun, which would still farther increase the strain upon the inner tube; and if it was able to resist the crushing pressure in the first instance, the heating of the inner tube would greatly augment the strain. He thought that was the reason why men, like Sir William Armstrong and the Woolwich authorities, who well knew what they were about, had not ventured to give that distribution of strain by shrinkage which mathematical reasoning would lead them to adopt. The Author would probably admit that the mathematical knowledge which he brought forward nineteen years ago would not have remained idle for so long a time if it could have been advantageously applied; but Dr. Siemens was clearly of opinion that in the construction of the Woolwich guns it was impossible to apply that reasoning properly. But the Author did not really intend to make a gun by shrinking on iron rings upon a steel core; he had brought forward a construction of his own, which had something very pleasing to recommend it at first sight. He took a lining tube, and bound upon it several layers of wire, increasing and varying the strain of the wire in such a way that when the inner pressure—the maximum powder pressure—was applied, each portion of the material bore the same amount of tensile strain. There again, however, the Author had not contemplated, or, at all events, had not brought forward, the result of the crushing action of that amount of binding force upon a comparatively small tube. The tube was made of cast iron, which resisted compression better than extension, and to that extent the construction might be very proper; but the powder pressure in heating the tube would, he apprehended, work a change in the tensions, the same as it would do in the iron coils, either causing the wires to be overstrained, or the tube to be crushed under that strain. The question was, however, whether it would be possible to put the material into any more satisfactory form. When the powder pressure acted, artillerists would like to distribute the force uniformly over the material; but they had to contend with the different portions under the two conditions of rest and of action from within. One great drawback to the Author's proposed system, which he himself admitted, was, that he had a resistance against bursting strain without resistance in the longitudinal direction. The Author remedied that by putting a very heavy mass into the breech piece of the gun, connecting that heavy mass with a protecting tube which did nothing towards resisting the bursting strain of the gun. This was a great drawback to that mode of construction. The amount of metal which actually resisted the bursting strain was probably not more than one-third of the total weight of the gun, the other two-thirds being used for a totally different purpose—to resist recoil and longitudinal action. Why should a wire coil be used? Was metal in the form of wire susceptible under ordinary circumstances of bearing any greater resistance than solid metal? Certainly not. The same strain per inch could be resisted by solid material. In the case of wire there was certainly this advantage, that inasmuch as the iron of commerce was a mixture, or concrete, of the metal iron and a glassy substance called cinder, if that glassy substance were mixed in an irregular way with the iron it would give no tensile strength at all; but inasmuch as the layers of glass were drawn out in a longitudinal direction, they did less harm than they would do if mixed promiscuously; and for that reason wire gave a larger resisting strain than solid metal. That, however, was not the case with steel. Steel wire resisted no better than solid steel. On the contrary, steel wire, while resisting no better per square inch, would give less

elongation than solid steel; but the Author would say, "You cannot with solid steel distribute your strains in the manner I tell you to do; there ought to be more strain near the outer layers, and less towards the inside of the gun." But there again he thought the steel maker would not be at a loss. If it was desired to distribute a compressive strain on the inner surface of the ring and a tensile strain on the outer surface, could that not be obtained in any other way than by cutting the whole thing up into wire and winding it round, thereby losing all the advantage of longitudinal resistance? He thought it was quite possible. If a cylindrical mass of steel were heated in a furnace to redness, and then, while the steel was in the furnace, a cooling action was applied inside—say a spray of water—would immediately cause a solid tube to be formed inside the mass of what was called set or cold metal. The inner diameter would remain the same, because it was governed by the general mass of the tube. The metal would shrink, not in the direction of the diameter, but in the direction of the thickness of the tube, which thickness would become less. If the cooling action went on while the outside was at its full temperature, layer after layer of metal would be formed at a gradually reducing temperature, all of which would be, under those conditions, in a state of perfect rest and equilibrium. After some time, perhaps an hour, there would be an outside temperature of say 600° Cent., and at the inside 100° Cent. The coil being taken out of the furnace and allowed to cool, the result would naturally be that in each layer a tension would be formed proportionate to the relative temperatures. The contraction on the outer surface would be very great, and it would act as shrinkage upon the rest. The shrinkage of the surrounding layers would be less and less, and the total result would be a tension which might be represented by a diagram showing a maximum positive strain near the outside of the gun, and a maximum negative strain in the metal nearest the inside of the gun, the neutral axis being somewhere midway between the two. The explosive action of gunpowder upon such a tube would cause the internal negative pressure to be transferred into a positive pressure; it would act on the larger diameter in a less and less degree; and, when the full pressure was acting, there would be a field of compression which might possibly be represented by a diagram showing equal positive tension throughout the mass. In that way it would be possible to obtain, by means of a single ring, all the advantages which the Author claimed for his wire system, with the additional advantage of having strength in all directions, it being unnecessary to amplify the gun after all the required strength was obtained, by two or three times the amount of dead weight. He believed that an inner tube would always be necessary; but it should not be a resisting tube. A tube of hard metal, surrounded by comparatively weak metal, appeared to him to be a great mistake, which was shared both by the Woolwich system and by the plan proposed by the Author. The inner tube should be of metal that accommodated itself entirely to the outer tube and to the necessities of the gun. It should be extra mild steel, which would expand or extend 30 per cent. without break in its continuity. Any hard metal, such as hard steel or cast iron, would be subject to such action and reaction as to bring about its final destruction; whereas a lining of metal that was like putty in its constitution, coupled with great strength, was, in his opinion, the proper lining of a gun surrounded, not by a series of rings, but by one ring or tube, in which the strains were in the first instance so arranged as to give each portion of the metal an equal strength when the maximum powder-gas pressure was applied.

Mr. Homersham thought Dr. Siemens was mistaken in stating that sections of steel wire would only bear the same tension as a section of bar steel of the same area. He had had occasion some years ago to try a great many experiments upon the tension of wire, and certainly an equal area of steel wire drawn $\frac{1}{16}$th of an inch diameter stood considerably more than double the tension of iron. When steel was tempered by hardening in oil before being drawn into wire, it had more than double the strength of bar steel of the same area. All the best pianoforte wires were so made.

Dr. Siemens observed that he had made a great many experiments with

steel in the form of wire and in the solid state. He had not, perhaps, explained as fully as he ought to have done what he meant with regard to wire being no stronger than solid metal. In speaking of steel wire he supposed it to be annealed wire, that had not been subjected to any hardening process; but if the wire were cold-drawn, or oil-hardened, its elastic range would certainly be greatly increased. Any kind of steel, the mildest and the hardest, yielded in the same manner and to the same extent by applying the same amount of weight per square inch. The only difference between hard and strong and weak steel was, that the weak or mild steel came sooner to the limit of its elasticity. If it was desired to compare steel wire with steel in bulk, the steel in bulk should be put into a similar aggregate condition. Sir Joseph Whitworth could produce steel to bear a strain of 80 tons or more in the bulk after he had oil-hardened it; and Dr. Siemens knew from experience that it was exceedingly difficult to get steel wire, even when oil-hardened, that could be depended upon to resist more than 80 tons per square inch; occasionally 100 tons, or even 110 tons, might be reached in very thin wire, owing to great success in the mode of hardening. In order to make a fair comparison, a metal in the form of wire should be compared with metal in bulk subjected to analogous processes, when it would be found that the absolute tensile strength was nearly the same, whereas the solid steel had generally the advantage of elongating to a greater extent than wire, before rupture took place.

Dr. HOPKINSON said he would not occupy time in expressing the agreement which he felt with the Author in his adoption of a deductive method in discussing this problem, but would at once proceed to point out one or two things in which he thought the detail of his work was open to a slight criticism. He had verified the Author's equations, with the exception of the last. In arriving at that equation the Author had considered a ring of material, and had remarked that that ring was under circumferential tension; and he inferred that it would be extended by an amount represented by that tension, multiplied by the length of the ring and divided by the modulus of elasticity. That would be perfectly true if the only stress acting upon the material were the circumferential stress; but with that there was a radial compression, and also a stress parallel with the axis of the gun, the last being no doubt small in comparison with the others. Taking those into consideration, the accurate expression for the extension of a ring length l would be $\frac{Tl}{K}+\frac{fl}{K}-\frac{Zl}{K}$ where T, f, and Z were respectively the circumferential tension, the radial pressure, and the tension parallel to the axis; instead of $\frac{Tl}{K}$ as assumed by the Author. K′ was a constant for the material. According to Wertheim $K'=3K$ for several metals.

Assuming this, he would correct the Author's calculation for the $4\frac{1}{2}$-inch gun, as follows. The tension of the interior of the steel tube, instead of being 16.97 tons, would be 18.2 tons. The tension of the interior iron ring would be diminished by about $\frac{1}{2}$ ton. The amount of correction was very small, and did not materially affect the results. The point was one which was easily set right, but that was not the case with the other point to which he would refer. In testing a rod of any material by direct tension, its limit of elasticity was said to be reached when it began to take a permanent set. If, in addition to the tension, there was a lateral compression, it was a question whether it would take a permanent set with the same longitudinal stress as before. So far as he knew, no experiments had ever been tried upon that point. It had, however, an application to the theory of guns. It was generally thought that a press cylinder or gun would begin to give way just when the maximum tension in the material was equal to the limit of elasticity, and it was tacitly assumed that the compressive stress perpendicular to that tension would not affect the result. He did not think that anything of the kind should be assumed. The ordinary results of testing by pure tensile stress ought not, he thought, to be applied to cases in which there was also a lateral stress. The same consideration applied to many other cases. In the shell of a boiler, for instance, there was not only

a circumferential tension, but a longitudinal tension, which ought to be considered in any calculation as to the probable pressure at which a boiler would burst. It appeared to him that that was a subject deserving investigation; and until more was known about it, it was impossible to say definitely what the real tendency to break at any point of such a structure might be.

Mr. Hadden observed, that the author had quoted Barlow's formula for the diffusion of light, and so applied it to guns as to show that a gun might be made stronger by weakening it, as he did, 90 per cent. on the outside. All gunmakers were, however, agreed that the bore was the weakest part, its strength depending on the tensile value of the metal, not on its thickness. Dr. Siemens, he believed, had made steel capable of resisting 100 tons pressure on the square inch, and others had reached 60, 70, or 80 tons; but in the case of the "Thunderer" explosion, details of which he had in his hand, the bursting strain exerted was even there equal to 350 tons on the square inch. But if, as he thought, the accident happened by expansion and contraction (for which no allowance was made in any gun that he had ever heard of), it was equal to an expansive energy of 120 tons per square inch on that portion of the bore exposed to heat. The character of the broken section in no way indicated a burst necessarily longitudinal in direction, but indicated a tranverse or shearing strain. It appeared to him to be begging the question to acknowledge that a gun could not be made that would not burst. Such a gun could be made if the bore was so arranged that it was not exposed nakedly to the powder-pressure and heat. It had been sought to provide against the weakness of the bore—as metal sufficiently strong could not be manufactured—by the use of pebble powder. The advantage of pebble powder was obtained by so enlarging the area of the charge that its effort was spread over a larger surface longitudinally, while it might be better effected transversely without chambering. The gun possessed a larger margin of safety by diminishing the pressure on the bore from 35 to 40 tons per square inch to about 16 tons. It was inconvenient, however, to have a charge 5 or 7 feet long, as in the case of the 81-ton gun, when a charge of equal energy could be put in the trousers pocket. It would be seen by the diagram (Fig. 1) that he had evaded the difficulty in question, so that the metal, instead of having the requisite 200 tons pressure per square inch to resist, was subject to a strain of only 40 tons. A gun to be safe ought to be prepared to resist a stress of at least 200 tons per square inch in the bore. The Author had explained, that when a gun was in a state of rest, a certain portion of the gun, say the area between A and N, was in compression and the rest in tension, but that when an explosion took place they offered mutual resistance. Thus as there were two rings, one of compression and the other of tension, there must always be a neutral circle where there was rest, and that point was at N, Fig. 1. If the compression ring could be suppressed altogether, and something put in its place that was perfectly inert, the area of resistance offered to the charge would be increased in the proportion of the areas of N to A, so that the pressure at N would be very moderate indeed, in fact, reduced to the average. Compression area might, therefore, as well be omitted, for he did not believe a durable or safe gun possible which converted + into − strains every time the gun was fired. As the metal of the present gun did not average 50 tons tensile strength, though it was necessary to make it resist a strain of 200 tons, the desired result could only be effected by an expedient. The space between the fictitious or inner bore, A, and the real bore, B, was to be filled in by, say, compressed paper, which was inert, and did not, like metal, carry its expansive energy with it. The paper rolls used by calenderers were harder and denser than steel. It might appear a paradox, but it was none the less true, that a thick gun was weakest with regard to expansion, and strongest with regard to pressure. There were, therefore, these two contradictory elements in the gun, and the question was how to meet them; he believed by insulation. He had not seen in any gun provision for expansion and contraction, although even at atmospheric changes of temperature the force of expansion was one half the force of the powder, say, 9½ tons. The steel tube had proved the necessity for pro-

Fig. 1

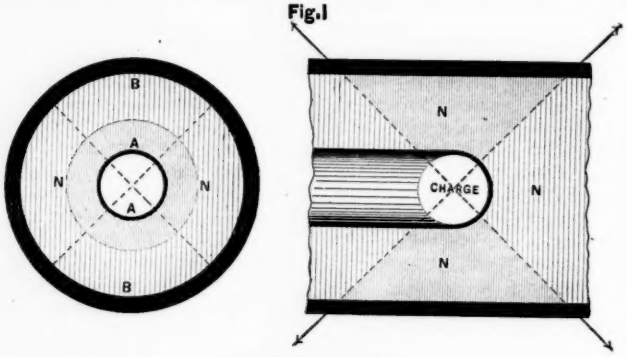

Diagram of 9-inch Gun.
A, fictitious bore, pressure=200 tons per square inch.
B, actual bore, pressure=40 tons per square inch.
N, neutral circle in an iron gun.
N, non-conductoric Mr. Haddan's gun.
NOTE.—The real bore is coned, to equilibrate the pressure.

viding for expansion by being elongated some inches out of the muzzle. He could not agree with the Author's method of calculating circumferential stress, just as if the gun was composed of a series of non-coherent 1-inch discs split in half and hinged. He could not see why Barlow's theory should not be equally applied longitudinally as transversely, in fact, spherically—or rather, hemi-spherically; the hemispheres being separated according to the length of the charge, as shown by dotted lines and arrows upon the diagram, where the charge was drawn spherically simply for illustration. The Author's calculation would apply if the gun were a cylinder open at both ends and the whole of the bore exposed to the maximum pressure simultaneously, as if it were filled with powder; whereas both ends were practically closed and the pressure was concentrated on a portion only of the bore. Mr. Haddan believed, judging from well known punching experiments, and also with the ogival-headed shot, that in a manner analogous to the distribution of pressure through, &c.; metals also transmitted pressure through their mass at angles varying with their molecular structure. The non-conducting lining would enable a weak metal to successfully resist any exaggerated strain required whether hot or cold, provided the area exposed to the force could be sufficiently enlarged to obtain the same result as in foundations. The other advantages of insulation were: it enabled gun-cotton or concentrated and more portable charges to be used without risk to the gun, since without diluting the charge with air-spaces, the same result was obtained, but radially as attempted by chambering in lieu of longitudinally. There was no waste of gas-power, from radiation or condensation. The heaviest gun could be constructed in loosely-fitting pieces and built up *in situ*. Flaws were not of consequence, except in the insulator, so that larger guns than even the 100-ton were not a remote contingency. Re-tubing could be performed in the field. The insulating tube could be supplied to existing artillery without reducing the caliber; and breech-loaders became feasible when the movable parts were not subject to heat. With reference to the "Thunderer" explosion he would merely say that he had, previous to receipt of data, published in 'Iron,' of the 22nd February, an exposition of his theory of shearing the gun by heat. He calculated that the heat derived from the explosion of the double charge would expand the bore 0.37 inch. It had been established that there was a radial increase of 0.34 inch in the diameter of the bore at the point of rupture. The rupture took place precisely at the spot where the gas-check of the leading shot rested; and he thought, that if the shot hesitated to move for an appreciable time after the gas-check was expanded, then the heat in the rear thereof would

have been about 1,400°; in contradiction to 35° of temperature forward. The antagonistic forces requisite to shear the cross-section of 13.4 square inches were here explained, by the irresistible force of heat expansion; in this case=350,000 tons. The gas-check gave the knife-edge or shear, which no amount of pressure could have done, since the rule, that the limit of continued resistance in a cylinder was measured by the tensile strength of the metal, was only true circumferentially, and would have manifested itself in longitudinal ripping, as in a boiler.

Commander W. Dawson, R. N., said that great strides had been made within living memory in the art of gun manufacture, not by soldiers or sailors, but by civilians, and chiefly by civil engineers. He gathered from the paper that it was desired to raise the art of manufacturing guns into a science. The system of construction hitherto adopted has been a kind of rule of thumb. With a certain quantity of metal to be manufactured into a gun, it was left to experience to determine what the length of the gun should be, the size of the bore, the dimensions of the projectile, and the charge of gunpowder. Guns were built by very practical and distinguished men, but they did not pretend to construct them on any scientific principle. The author had quoted a book to which the Secretary of State for War had put his name; but it should be remembered that that book had been written by certain young gentlemen who had come straight from the Academy at Woolwich, and not by Sir William Armstrong, or by any of the gun manufacturers. The fact was, the guns were built first and the mathematical formulæ found afterwards. What ought artillerists to exchange for the rule of thumb that had hitherto been adopted? The formulæ of mathematicans? Which mathematicans were they to follow? The author had pointed out that Professor Barlow had said one thing, Dr. Hart another, and he another; and, since the discussion had commenced, two or three mathematicians had suggested other formulæ. Should gun-making stand still until mathematicians had determined the right algebraical expression? He had great respect for mathematicians, but he agreed with Dr. Siemens that mathematics, while a good servant, might be a very bad master. All the formulæ to which reference had been made were very well, if the values of the different letters could be obtained; but if those values were not ascertained by experimental data, the formulæ were simply so much juggling with letters of the alphabet. Of course the Woolwich system was theoretically wrong, and no one knew that better than the constructors of the guns. But they were, perhaps, practically the best available in the present state of metallurgical knowledge. A hard steel tube was put inside, and soft metal outside. What could be worse than that? Of course the soft metal outside expanded much more than the tough steel inside, and if it was overstrained it would get the "set" first. It was, however, necessary to look at such matters practically. If wrought iron were put inside heavy ordnance, as in Sir William Palliser's system for light guns, what would happen? It would not stand the erosion, and the interior of the bore would be guttered and soon destroyed. It might be said: "Why not put on steel jackets outside?" As far as he had been able to gather from the progress made in late days, makers were constantly improving the manufacture of steel, so that probably in a few years they would be able so to manipulate steel as to produce any amount of ductility, or tensile strength, or any other quality, that might be required. He looked forward to the possibility of putting outside, not wrought iron jackets, but steel jackets. It might be said that the plan had been already tried; and so it had. In Krupp's guns a steel jacket was used. It could not be urged that the Germans knew nothing of war, and never fought; or that they were a spendthrift nation, who liked to pay double the necessary price for their weapons. At all events, *prima facie*, the expensive Krupp guns, which were steel, and were bought in preference to coil guns by every nation on the continent except France, had something in their favor. He had not read much on the subject of late, but a few years since he read a defence of the Woolwich system, based upon an attack on the Krupp system, and it ended in his being nearly converted the other way. The Author had described a gun which he did not wish

to criticize unduly, because he felt most grateful, as a sailor, to civilians who devoted attention to subjects of this kind, and it was to them that the nation had to look for future progress. But looking at the gun, he did not know how the dead weight at the breech was going to hold on to the barrel when the gun was fired with heavy charges. The chase of the gun appeared not to have sufficient strength, and it had not been prepared for the accident of a double charge. It had hardly strength enough for a single charge. The weight ought to be carried farther forward. A space of 10 feet was occupied by the two charges in the Thunderer's 38-ton gun, and there was only 6½ feet of free space. He had not been able to understand from the paper how the gun was to be practically constructed, or how the tensile strain was to be equably applied. If the plan were of value, he should have thought that during nineteen years some talented engineer would have applied the process to strengthening the cylinders of steam engines. Reference had been made to the explosion of the 38-ton gun on the Thunderer. It so happened that six years ago he had read, at the Royal United Service Institution, a Paper upon that very gun, and he had traced certain injuries, not to the gun itself, but to the shot within it. He had during two or three years examined a great number of shot that had been recovered after being fired, and had found on the studs, which were alone supposed to touch the gun, certain marks which ought not to be there if the shot had come out fair and straight from the gun. They were wedge-shaped, or misshapen in various other ways. He was not allowed to see the guns themselves, being a sailor, and not a soldier, and he accordingly examined the published official reports with regard to the position of the marks inside the guns, and ascertained that a large number of the guns were injured by their own shot in coming out. The majority of these injuries took place not, as might be imagined, in the powder-chamber, but in the portion of the gun traversed by the shot—in most cases within 10 or 12 inches of the seat of the shot—and these "cracks," "burrs," and other marks in the bore, were directly traceable to the studs; in fact the marks within the gun and the marks on the studs agreed. It was no wonder then that English guns had "decidedly the lowest velocities," according to the official reports from Woolwich. When two similar guns were fired with similar shot and similar charges, and one shot came out at a maximum speed and the other at a reduced speed, what was the cause? The same force was developed in the powder chamber, and applied to the base of the two projectiles, but when the two shots came out of their bores they came out with different energies. It was evident that the differences of the momentum of the two escaping shot represented the force or power absorbed within the gun in making the marks, and in injuring its own shot and the gun itself. Certain 400-lb. common shell were found to have their walls compressed by the studs wedging between the shell and the bore, and these walls had to be strengthened by reducing their powder capacity. Another thing he had observed was, that there was no relation between the powder pressure in the chamber and the velocity with which the shot came out of the gun. On the contrary, there was a difference of from 20 tons to 60 tons pressure per square inch on the base of the projectile, and yet the shot came out with the same velocity as before; or sometimes it was reversed, and it came out with less velocity when there was a high pressure on its base than it did when there was a low pressure in the chamber. These were facts, taken from the published reports, and it was upon them that he had examined the position of the injuries on piles of recovered projectiles and in the reports on damaged guns, and had based the strictures he had made six years ago, on the gun that had now failed. His belief was, that whatever fault existed was not in the gun nor in the gunpowder, but in the shot, the exit of which from the bore was obstructed by the wriggling motion incidental to its being balanced on two points nearly under the center, and to its having a rifle-bearing of less than 1 inch in each groove. The explosion in the "Thunderer" was not the first that had occurred. A 68-pounder of Sir William Palliser's construction had exploded, and Captain Dawson had endeavored to dis-

cover the reason. He believed that the powder charge had only a secondary part in that explosion. The accident had nothing to do with the construction of that gun, for the gun was stronger than it was before; the sole cause was the wriggling of the studded shot in its effort to escape from the charge. The peculiar way in which the shot was balanced, in unstable equilibrium upon two points, prevented it sliding out as if on rails. Two bronze guns were exploded in India during experiments, and the marks within them were noted as the firing proceeded. Those marks were clearly shown to have been made in front of the seat of the studs by the studs, and not by the powder. When the accident on board the "Thunderer" was first reported in England, one of the causes assigned was, that there was an air space in the gun, and some authorities stated that that would burst the gun. In his experience as a naval officer he had had the responsible charge of firing from eight thousand to ten thousand heavy shot of the period, and he was quite sure that several hundred of them must have been fired with a space between the shot and the cartridge. It was difficult to get a heavy gun out in a rolling ship without a jar, which shifted the shot from its place; but he had never heard of a gun exploding, or any serious injury happening, from that cause. It was found by experiment, that if a gun were inclined 14° the shot would slip out a certain distance; and a roll of 14° for a ship was nothing, so that such a slip must be of frequent occurrence. He did not believe that an air space would lead to an explosion; but, if it were otherwise, it was disgraceful that he should be left in ignorance of the fact—that having had the firing of so many heavy guns under his charge, he should never have been told of this danger. It was often wanted to explode enemy's guns, and it was not known how to do it; so that the discovery that a little air was all that was wanted to destroy captured guns might be really useful. Another reason had been recently given, that there were two charges in the gun of the united weight of 195 lbs. of powder and 1,315 lbs. of shot. Many persons might think that that circumstance was rather astounding, but it was a thing that must have frequently happened before. When fifty or sixty guns were being fired close together, it was difficult to tell whether one particular gun had gone off or not, so that in former naval actions many guns must have been so treated. The occurrence happened sometimes even in ordinary quiet target practice, and he had observed it in his own personal experience. On one occasion, when firing on board an eighty-gun ship, as he went along the battery he noticed that the rammer of a gun was not inserted to the proper distance, and he called out "Not home." The captain of the gun replied, "Yes, it is home." Captain Dawson looked in, and found the gun was half full of charges. A worm was then brought and three charges were pulled out, the fourth having been about to be put in. What would have happened if the gun had been fired with the four charges? There would have been a little kick, and that would have been the end of it. It was one of the old cast-iron guns, that were nowadays despised. The reason why no harm would have been done was, that there was no shot in the gun. It was the shot that did the mischief. If, however, the shot could be made to glide away from its seat without wriggling, no mischief would be done; the gases would escape, and no explosion would take place. Whether the "Thunderer's" 38-ton gun exploded by detonation or not, the chances of its doing so would have been much lessened, if the shot could have slipped out of the way on rails with ease, instead of having to give several preliminary wriggles before starting fair. When investigating the question a few years ago, he had been puzzled at finding that a 10-inch gun, which had been boasted about a good deal at Woolwich, had been fired a great many rounds without being battered to pieces by its own studded shot, like other Woolwich guns. He found on inquiry that the studs had been clipped off the shot, so that their whole length rested upon the bore and they were able to come out freely, and the gun at the end of the firing was almost as good as it was on the day when it was made. The great evil of English guns was the extremely low velocity of the projectiles when fired, so that they hit very weak blows. Whilst the German guns had been subjected to the rough usages of

war, the English heavy guns had never been tried in action except once, or in any way similar to a naval action. The only case was that of the "Shah" and the "Huascar." The "Huascar" was an ironclad, with as thin sides as the old "Warrior." After firing 16 tons of shot and about 431 rounds at her, the result was that one man on board the "Huascar" was killed. It should always be remembered that fifty shot had to be fired for one effective hit.* That was the practical way in which the question should be regarded. A gun was wanted that would fire fifty rounds in as short a time as possible, so as to make the one effective hit quickly before the enemy could make his hit. Of course the gun got heated; then the interior expanded and the exterior expanded, and if the exterior did not expand at the same rate as the interior, but took a "set" sooner, the evil complained of would occur.

Admiral SELWYN thought the Author had shown quite sufficient energy and determination, and *prima facie* argument, to justify him in seeking and obtaining, what all such matters deserved, an experimental trial. When it was stated that a gun could be built for one quarter of the cost of the existing guns, a good case was made out for a trial of the system. Guns of 100 tons had been sold to the Government for £16,000 each by Sir William Armstrong, and the nation would certainly desire to economise in that direction. He was not, as a sailor, competent to decide upon the question of the tensile strain into which a coil of wire would be thrown by the discharge of powder inside it; but he could hardly believe that an engineer would tell him that an effect might be greater than its cause. If the pressure gauges had shown an extreme pressure, of from 24 to 27 tons per square inch, he did not see how the wire could get a greater pressure from any other source. Powder gas was made under certain conditions; it gave a fair record of its pressure wherever it found anything to crush; and he believed the authorities at Woolwich were satisfied that that strain under ordinary conditions was not practically exceeded in a gun. Sir Joseph Whitworth had made a gun of solid steel, in which he could burn away the whole charge through the touch-hole, the gun being plugged up. It was time attention should be paid to making guns stronger and of better metal; and, if the Author had been able to advance from two laws which were not applicable to the case to a law which was applicable, being only subject to a slight error of 3 or 4 tons, such a great theoretical advantage ought to be received with pleasure by gun manufacturers. There was a time when strong guns bore the pressure and the number of fires required from them with perfect stability. The bursting of an iron gun was rare, unless there was some extraordinary maltreatment. A new mode was then discovered of rifling guns, and thereby getting greater accuracy and a longer range with a smaller diameter and greater length of shot; but it was ascertained that, in order to produce the rotation of the elongated projectile as well its projection, without greatly increased strain, there had to be a less powder charge; and, indeed, even with less powder charge, strains were set up which could not be met by built-up guns. The powder was then changed for one resembling the weakest powders made in China and in India. It was good enough for its purpose, because it burnt over a long time, and in a long gun it answered very well. The guns next had to be made longer in consequence of the weak powder, and then it became necessary to enlarge the casemates to receive the longer gun. It was next ascertained, according to the late report of the Artillery Committee, that, unless the gun was washed out carefully after every round, the shot tended to jam up against the fouling. If the guns were stronger, good powder could be used, and they might be shorter, and there would be no smoke in the casemates, which led to the aban-

*The old rule was, "The weight of the gun in shot to kill one man," or two effective hits per hundred shot fired. The shot fired at the "Huascar" were:

"Shah" fired							
	32	projectiles of	250 lbs.	from	12½-ton guns	=	3½ tons.
"	149	"	100	"	6½ "	=	6½ "
"	60	"	64	"	3½ "	=	1½ "
	241						11½ "
	190	"Amethyst"	64	"	3½ "	=	5½ "
Total	431	"			Total		17 "

The range varied from 300 to 2,000 yards. The firing was spread over 2½ hours. Four 250 lbs. projectiles, two 100 lbs. projectiles, and four of the 64 lbs. projectiles struck the hull of the "Huascar," = 10 shots out of 451, *i.e.*, better than the ordinary rule of discharges, 2 per cent. or one hit in fifty. There were two perforations of the hull, and one man was killed and one wounded by these 431 projectiles.—W. D.

donment of breech-loading artillery for ships thirty to forty years ago. He remembered seeing the first gun of that kind tried, and it filled the whole deck with a peculiar kind of smoke. When the breech of the gun was opened in the confined space after firing, the draught of air set up changed the carbonic acid gas, which would be the result of perfect combustion, to carbonic oxide gas—probably because the oxygen coming in took up some of the unburnt carbon remaining in the gun, and thus made CO instead of CO^2—which was of a very stifling nature. There was another reason for not using bad powder. It had been discovered that the same kind of powder would not do for more than two or three kinds of guns; that one kind was required for a mortar, another for a howitzer, another for a long gun, and so on. That of itself introduced objectionable complexity into the subject of powder supply in action. Sometimes, again, the whole charge ignited, while at other times many grains were unignited, and that caused a difference of range. All those were results of abandoning correct principles. The best materials should be employed for making powder gas, and the authorities ought not to be induced, by any deficiency in the machine in which the gas was used, to give up the manufacture of it from the best materials, and in the manner most calculated to produce perfect combustion. With regard to the closure of the bore, to which reference had been made, some years ago his attention had been drawn to a clever use of the cylindrical wedge by Major Penrice, who showed that, by taking a cylinder about the size of the shot belonging to a gun, and dividing it into three wedges, the base of the central wedge being towards the powder, no explosion of powder could drive that out of the bore; there was an irremovable wedge against powder impact, but a perfectly removable one against the slightest push. Thus the whole longitudinal strain was transformed into circumferential strain, if there was radial strength enough; and there should be no necessity for the rather cumbrous machinery that had been brought forward.' He did not find fault with it, as some had done, because he doubted whether the tensile strain could be properly calculated on the connecting bars outside. He had no doubt that the Author had calculated the tensile strain on his bars sufficiently accurate to dispose of the question. It was important to have as light a gun as possible. People said: "If you have a light gun you will have a heavy recoil." Colonel Moncrieff had shown an accurate and scientific mode of doing away with that objection. He had utilized the recoil, instead of allowing it to destroy the carriage, by making it bring down the gun into cover. He did not fear firing with the lightest gun that could be given to him. It was in a scientific direction that the objection was to be removed, just as the objection to bad powder was to be removed by increasing the strength of the gun, in whatever manner. No doubt solid steel guns would be more cheaply manufactured than with almost any other material. He did not know whether Dr. Siemens could produce a gun of solid cast steel, tempered as he had described, and as Captain Rodman had first described for cast iron guns, at the same price as the Author had spoken of. But, even if that could be done, he saw no objection to having experiments with the Author's system; and he was quite sure that, when the House of Commons and the nation thoroughly understood that money spent in experiments on those subjects, under proper supervision and advice, was the cheapest and wisest expenditure, complaints would no longer be made by so many inventors that they could not get their principles worked out to the bitter end. Engineers, who well knew how much they owed to experiments conducted by members of their own body at great expense to themselves, and how the nation had benefited by them, would, he hoped, always be advocates of liberal votes for experiments. He was sorry to hear the Author say that he had no longer any other than a scientific interest in the question, because he knew that any invention in which only a scientific interest was felt was like an orphan child. Nobody put any money in it, because there was nothing to be got out of it. So many discussions on different systems would not have been heard in the Institution of Civil Engineers, if it had not been felt by the speakers that

they had a property in the inventions, and that they could do something better than anybody else could do it. That was a stimulus which he longed to see applied throughout the nation—a stimulus which would produce new industries, new trade and commerce; and, as soon as reasonable protection could be given to inventions, which were the seed-corn of industry, it would be found that England would not be behind America in its powers of production.

Captain NOBLE observed, through the Secretary, that with many of the Author's theoretical views he concurred. As far as he remembered, the formulæ he used were deduced (in an able memorandum by Mr. Brooks, which formed part of the Author's former Paper) from the expression for the strength of cylinders or hoops in common use; and these formulæ, or similar ones, no doubt underlaid the principles upon which all built-up ordnance of the present day was constructed. He could not, however, agree that the description given by the Author of the Armstrong mode of construction was at all in accordance either with the views or the practice prevailing at the Elswick works. In all cases when a gun had to be designed, the maximum pressure to which it was to be subjected was first determined. The dimensions and shrinkages of the coils were then fixed, the calculated shrinkages being greatly modified by the results of a long series of experiments carried on during many years, chiefly by Sir W. Armstrong personally. Rightly or wrongly, proof had, it was believed, been obtained that, with the shrinkages applied, greater strength was got than would be the case were the guns made in accordance with the rules recommended by the Author. He was also unable to concur in the view, that there was any ground for supposing that by the explosion of a charge of powder in a gun, that gun was strained to double the extent it would be were the same pressure gradually applied. As far as he knew, in almost all cases where material had failed under the employment of suddenly applied strains, such failure had, in his opinion, been chiefly due, not to the suddenness of the application, but to the fact that, from the *modus agendi*, energy in one form or another had been generated, so that, in addition to the ordinary strain, the material had had to bear the extra strain arising from this cause. So far from thinking that a gun, under the actual circumstances, would only stand half of what it would do were the pressure applied more gradually, he believed that many guns, owing to the almost infinitesimal time during which the highest pressures acted, withstood pressures that would undoubtedly cause their failure were such pressures of longer duration. As regarded the mode of manufacture for which the Author contended, he might mention that the same idea was independently proposed by the late Mr. Brunel, when attention first began to be directed to rifled ordnance, and that he asked Sir William Armstrong to construct a gun on this principle. The work was commenced, and would have been carried out had not Mr. Brunel countermanded the order, on hearing that a patent had been taken out by the Author for the same thing. It was afterwards found that a still earlier patent had been taken out by Captain Blakely; and he had been informed that a gun was made and tried in America on the same principle, under the direction of Professor Treadwell. Attention had been frequently given to the subject at Elswick, but the advantage of the system had been considered much more questionable on practical than on theoretical grounds.

Mr. CANET said, in the course of the discussion mathematicians had been taunted with being unable to agree amongst themselves as to the accuracy of the various formulæ propounded for giving the strength of a tube subjected to an internal pressure, and therefore it was argued that mathematics had nothing to do with the construction of ordnance, and that the well-known rule of thumb was the only safe guide. He could not agree with this, as it was not disputed that Professor Barlow's law was only approximate; but, if no notice were taken of the valuable indications given by theory, it would not be possible to judge accurately of the strength of guns of various systems of construction, which he considered was the subject under discussion; and the manufacture of ordnance would go on as at present, every manufacturer pretending to make the

most reliable guns. The strength of cylinders had been fully investigated for the first time by Monsieur Lamé, who gave its true theory in his "Legons sur la Théorie Mathématique de l'Élasticité des corps solides." From this theory was deduced the following formula, for the internal equilibrium of a cylinder subjected to interior and exterior pressures f_0 and f_1,

$$t=f_1\times\frac{\rho^2}{y^2}\times\frac{R^2+y^2}{R^2-\rho^2}-f_1 \times\frac{R^2}{y^2}\times\frac{\rho^2+y^2}{R^2-\rho^2} \quad . \quad . \quad . \quad (1)$$

In order to avoid confusion he had kept the same notation as the Author. This formula depended upon the admission that the metal forming the cylinder was equally elastic at every point of its mass, and in every direction—in other words, that the metal was homogeneous; so that, if the formula could not be applied to a metal, it would not be the fault of the formula but of the metal itself, which was not as perfect as was admitted in the formula. But, even in this case, the problem had been solved by an able officer, General Virgile, of the French artillery. He had assumed in his calculations three moduli of elasticity, varying according as the metal was taken radially, circumferentially, or longitudinally, thus allowing for all the variations that were likely to be found in practice. This involved long calculation, and for practical purposes formula (1) was sufficient. The Author, in his calculations of the strength of a gun, had taken a certain definite shrinkage, a certain interior pressure, and a certain modulus of elasticity. Should these three elements of the calculation not exist in the gun under consideration, in the precise form that the Author had assumed, the calculations would have to be re-made. It was, for these reasons, difficult to compare one system of construction with another, in following the Author's method of calculation. But this could be done, he thought, simply as follows. It was obvious that in a gun no part ought to be strained, before and during explosion, beyond the elastic limit of the metal of which it was composed. If this condition was not fulfilled, the gun would be injured permanently at each round fired, and failure would be imminent. It was equally obvious that, to get the maximum resistance out of a built-up gun when the interior pressure reached a certain point, the interior surface of every layer or section of which the gun was composed (which was the part of each layer or section most strained) should be stretched at the same time up to its elastic limit. He therefore called the strength of a gun the maximum pressure, per square inch, that could be exerted in the bore without straining any part of the gun beyond its elastic limit, the shrinkage being disposed during manufacture so as to make each layer or section work to the utmost whilst the structure was under that pressure; and he had assumed in his calculations that these conditions had been fulfilled. To have the internal normal pressure at the inner surface of the $(n+1)$th ring, let in equation (1)

$$t=T_n$$
$$f_0=f_n \qquad f_1=f_{n+1}$$
$$\rho=R_n \qquad R=R_{n+1}$$

then

$$f_n=T_n\frac{R_{n+1}^2-R_n^2}{R_{n+1}^2+R_n^2}+\frac{2f_{n+1}\,R_{n+1}^2}{R_{n+1}^2+R_n} \quad . \quad (2)$$

R_n and R_{n+1} were the radii of contact of the ring under consideration; T_n was the elastic limit of the metal of the ring; and f_n the normal pressure on the interior surface of the ring.

For the last ring, $f_{n+1}=0$.

From equation (2) it would be easy to calculate f_n, then f_{n-1}, then $f_{n-2}\ldots f_0$.

He had made these calculations for several guns, taking for the elastic and ultimate stress of steel the figures given in Table I. These materials were used

TABLE I.

	Stress per Square Inch.	
	Elastic.	Ultimate
	Tons.	Tons.
Wrought iron used at Woolwich	11	23
Oil-tempered steel for tubes	25	50
" " jackets reheated and cooled in oil	21	40
Oil-tempered steel for tubes " " jackets reheated and cooled in atmosphere	18	35
Soft steel for exterior rings	16	32

at the London Ordnance Works, with which he was connected, and the results quoted were obtained by direct experiment. The solid 10-inch guns, of cast iron, wrought iron and steel, given for comparison, were supposed to be made of the same dimensions as the Woolwich gun. The 80-pounder B.L., 10-inch and 12-inch built-up steel guns, were of the construction originally adopted by the late Captain Blakely, R.A.,* and now manufactured by Mr. Vavasseur, M. Inst. C.E. They consisted of a tube, jacket, and one or two layers of steel rings according to the caliber. He had selected these guns because they were those with which he was best acquainted.

It was easy to demonstrate mathematically that a sudden strain was equivalent in effect to double a steady strain of the same amount. On the other hand, Mr. Kirkaldy found by experiment that the effect of a sudden strain was equal to 81.5 per cent. of a steady pull. This depended, of course, upon the suddenness of the application. It seemed obvious that in a gun the strain was applied more suddenly than could be done in experiments, so that a strain of 81.5 per cent. was too much, and of 50 per cent., which was the theoretical amount, too little. In taking a mean between the two, say 66 per cent., or $\frac{2}{3}$, he would probably be nearer the truth, and the figures of the fourth column of Table II. had been so calculated. They showed

TABLE II.

	Greatest Diameter over Chamber.	Weight of Gun.	Strength of Gun per Square Inch in bore.		Initial Compression of Bore per Square Inch.
			Statical.	Dynamical	
	Inches.	Tons.	Tons.	Tons.	Tons.
9-pounder gun, Woolwich construction, mark II.	9.75	0.4	19.94	13.3	+ 0.9
Same gun, built up with steel	"	"	29.17	19.4	—10.3
80-pounder B.L. built-up steel gun Vavasseur construction	32.5	4.0	34.32	22.9	—15.6
7-inch gun, 90 cwt., Woolwich construction, mark I.	26.0	4 5	26.21	17.5	— 5.3
Same gun built up with steel	"	"	35.60	23.7	—16.3
10-inch solid cast-iron gun	45.0	18.0	5.44	3.6	0
" " wrought-iron gun	"	"	9.97	6.6	0
" " steel gun	"	"	14.50	9.6	0
" gun Woolwich construction, mark I	"	"	23.84	16.0	— 1 3
10 inch gun Woolwich construction, mark II	"	"	28.62	19.1	— 6.6
10-inch built-up steel gun, Vavasseur construction	41.0	16.3	39.71	26.5	—19.8
12-inch 38-ton gun, Woolwich construction	57.5	38.0	30.82	20.5	— 8.6
12-inch built-up steel gun, Vavasseur construction	45.0	27.0	36.13	24.1	—16.7
80-ton gun, Woolwich construction	72.0	80.0	28.49	19.0	— 6.5

clearly the maximum pressure that could be used, according to the system of construction, without injuring the gun permanently at each round. If greater pressure than those indicated for each gun were used, in order to obtain greater effects, then the life of the gun was limited to a certain number of rounds. Having found the strength of a gun, the next thing was to see that, in its initial state, the interior surface of the bore was not compressed to such a point as to be injured; and it was this condition that lim-

* Captain Blakely was, he believed, the first to construct built up guns of steel. In the Exhibition of 1862 he exhibited several built-up steel guns. One, made at Liverpool, was of 8 inches bore; another, of 6 inches bore, was made by Mr. Vavasseur; while all the guns exhibited by Mr. Krupp were made of solid forgings, and had even the trunnions forged on.—G.C.

ited the strength of any gun. It was highly probable that the compression necessary to injure the tube of a gun was very great. The experiments made in crushing pieces of metal, that were allowed to give way literally, did not certainly apply to cylinders that remained similar to themselves when under pressure. The results of his calculations were embodied in Table II. He found that the initial compression of the bore of the 9-pounder Woolwich gun was positive. By this expression he wished to convey the idea, not that the bore of the gun was in a state of initial tension, but that, with the proportions of steel and wrought iron adopted, it was impossible, on the basis of his calculations, to obtain the maximum amount of resistance from the materials employed; therefore the gun was slightly weaker than a solid gun made of the same steel as the tube. But if a steel jacket was substituted for the wrought-iron coil, the strength was greatly increased, and the gun would probably be able to stand well with any charge of powder, as it was easy in field guns to keep the pressure of the gases under 15 tons per square inch. He quite agreed with a previous speaker, that the Author had made an omission in not taking into consideration the lateral deformation of a cylinder when under pressure and submitted at the same time to a longitudinal strain; but the question of the shrinkage had not the importance that had been attached to it by the Author—on the condition, however, that the shrinkage was not less than that which was given by the theory, and not so great as to bring the outer layer on the point of breaking. Supposing it was so in the 10-inch Woolwich gun, Mark II., and that the interior pressure was equal at each round to the strength of the gun, viz., 19 tons, too great a shrinkage meant that there was more tension on the outer rings than was wanted; consequently the rings would be strained beyond their elastic limit; they would be stretched permanently at each round, and the shrinkage would decrease accordingly till the gun was theoretically perfect. And if from that moment the pressure was kept under 19 tons, the gun would stand for ever, as far as the tangential strength was concerned; but if the interior pressure was above 19 tons, the injury would go on with the firing, till the tube, not receiving a proper amount of support from the external coils, failed by cracking. A gun of any construction subjected to an interior pressure greater than its strength, as explained, must ultimately fail. This was so well understood by the French, that it was a rule with them to test to destruction a gun of every caliber or system of construction that had been approved of for the service. They found experimentally the number of rounds that it would stand in actual practice, and therefore, when a gun issued for service had fired the number of rounds considered consistent with safety, it was withdrawn and condemned. If it was necessary to have guns of greater power of internal resistance than those shown in Table II., an increased thickness of metal must be added to the walls of the gun, or a stronger metal must be used, or a different system of construction—such, for instance, as that proposed by the Author—must be adopted. As to the question of heat, it did not at all affect the principles of building up; but simply increased the tension of the outer rings. Table II. showed the great difference in the strength of guns made of iron and steel, or entirely of steel. It was a curious fact that steel as a material for guns had been adopted in nearly every European country except England, where the manufacture of that metal was the best. Steel guns were now used by France, Germany and Russia, also by Italy for field guns. The guns of the French navy had long been made of cast iron and steel. The theory proved what was already known by practice, that these guns were inefficient to resist high internal pressure, and, notwithstanding their great cheapness, and the alleged variable quality of steel made by French manufacturers—probably due to their want of experience in the manufacture of the large masses required for this purpose—guns built up entirely of the metal had been adopted for some time.

It might, perhaps, be interesting to mention, that wire guns had been experimented upon by the French Government. The system tried was that proposed by Captain Schultz, of the French Artillery. Four guns were made—two field guns of 3.15 inches bore, and two

7½-inch guns, one lined with steel and the other with cast-iron. One of the field guns had fired, up to the time of the information he had received, 433 rounds, with projectiles weighing from 17.6 lbs. to 30.8 lbs., the initial velocity ranging from 1510 feet for the heaviest projectile, to 1887 feet for the lightest. The 7½-inch gun lined with cast iron fired nearly 300 rounds. After the experiments no enlargement could be detected in the powder chamber. In Captain Schultz's design for a 24 centimeters (9½ inches) gun, there were over the chamber 34 rows of .0945-inch steel wire, and 17 rows only in front of the trunnions. The breech mechanism, which was on the French system, was put into a separate breech-block connected with the trunnion hoop by twelve 4.4-inch steel bolts. The general dimensions of the gun were given in the drawing he exhibited. It would, he thought, be conceded by all, that guns must be built up; and if each layer of wire was considered as a steel tube whose tension could be regulated and varied at will, the system of construction to which the Author had invited attention seemed to provide tubes far stronger than any that had yet been made; and so far as practice had gone it showed that those guns were as strong as theory said they must be. When it was considered that steel wire was equal to an elastic stress of 80 tons, and a breaking strain of 120 tons per square inch, guns of this construction must have a great future before them.

ELEMENTS OF THE MATHEMATICAL THEORY OF FLUID MOTION. PART II.

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I.

In certain problems in the theory of fluid motion the integration of the differential equations which present themselves is rendered less difficult by the introduction of curvilinear co-ordinates. We shall now turn our attention to the transformation of the differential equations of fluid motion by the aid of such systems of co-ordinates.

Let these be three surfaces represented by the equations

$$u=f_1(x, y, z)$$
$$v=f_2(x, y, z)$$
$$w=f_3(x, y, z).$$

Here x, y, z are variable co-ordinates and u, v, w are parameters which are constant for any surface; by varying any one parameter we obtain a family of corresponding surfaces. To given values of x, y, z will correspond definite values of u, v, w; and conversely, if u, v, w, are given the values of x, y, z may theoretically be found. Suppose that u, v, w satisfy the following equations:

$$\frac{dx}{du}\frac{dx}{dv}+\frac{dy}{du}\frac{dy}{dv}+\frac{dz}{du}\frac{dz}{dv}=0$$
$$\frac{dx}{dv}\frac{dx}{dw}+\frac{dy}{dv}\frac{dy}{dw}+\frac{dz}{dv}\frac{dz}{dw}=0$$
$$\frac{dx}{dw}\frac{dx}{du}+\frac{dy}{dw}\frac{dy}{du}+\frac{dz}{dw}\frac{dz}{du}=0$$
$$dx=\frac{dx}{du}du+\frac{dx}{dv}dv+\frac{dx}{dw}dw$$
$$dy=\frac{dy}{du}du+\frac{dy}{dv}dv+\frac{dy}{dw}dw$$
$$dz=\frac{dz}{du}du+\frac{dz}{dv}dv+\frac{dz}{dw}dw$$

Suppose now that we define three functions U, V, W as follows:

$$\frac{1}{U^2}=\left(\frac{dx}{du}\right)^2+\left(\frac{dy}{du}\right)^2+\left(\frac{dz}{du}\right)^2$$
$$\frac{1}{V^2}=\left(\frac{dx}{dv}\right)^2+\left(\frac{dy}{dv}\right)^2+\left(\frac{dz}{dv}\right)^2$$
$$\frac{1}{W^2}=\left(\frac{dx}{dw}\right)^2+\left(\frac{dy}{dw}\right)^2+\left(\frac{dy}{dw}\right)^2$$

Now writing the above equations in the form

$$dx = \mathrm{U}\frac{dx}{du}\frac{du}{\mathrm{U}} + \mathrm{V}\frac{dx}{dv}\frac{dv}{\mathrm{V}} + \mathrm{W}\frac{dx}{dw}\frac{dw}{\mathrm{W}}$$
$$dy = \mathrm{U}\frac{dy}{du}\frac{du}{\mathrm{U}} + \mathrm{V}\frac{dy}{dv}\frac{dv}{\mathrm{V}} + \mathrm{W}\frac{dy}{dw}\frac{dw}{\mathrm{W}}$$
$$dz = \mathrm{U}\frac{dz}{du}\frac{du}{\mathrm{U}} + \mathrm{V}\frac{dz}{dv}\frac{dv}{\mathrm{V}} + \mathrm{W}\frac{dz}{dw}\frac{dw}{\mathrm{W}}$$

If then dx, dy, dz be taken as the co-ordinates of a point infinitely near (x,y,z) with reference to a system of co-ordinates whose origin is at x, y, z, we see that

$$\frac{du}{\mathrm{U}}, \frac{dv}{\mathrm{V}}, \frac{dw}{\mathrm{W}},$$

may be taken as the co-ordinates of the same point with reference to a system of rectangular co-ordinates whose origin is the same, but whose axes make with the axes of x, y, z angles whose cosines are the coefficients of these quantities in our equations. We have further, as is obvious

$$ds^2 = dx^2 + dy^2 + dz^2 = \left(\frac{du}{\mathrm{U}}\right)^2 + \left(\frac{dv}{\mathrm{V}}\right)^2 + \left(\frac{dw}{\mathrm{W}}\right)^2$$

and again for element of volume $d\tau$, supposing, as we do du, dv, dw, U, V, W, all positive,

$$d\tau = \frac{du}{\mathrm{U}}\cdot\frac{dv}{\mathrm{V}}\cdot\frac{dw}{\mathrm{W}}$$

Solution of the above equations gives us

$$\frac{du}{\mathrm{U}} = \frac{\frac{du}{dx}}{\mathrm{U}}dx + \frac{\frac{du}{dy}}{\mathrm{U}}dy + \frac{\frac{du}{dz}}{\mathrm{U}}dz$$
$$\frac{dv}{\mathrm{V}} = \frac{\frac{dv}{dx}}{\mathrm{V}}dx + \frac{\frac{dv}{dy}}{\mathrm{V}}dy + \frac{\frac{dv}{dz}}{\mathrm{V}}dz$$
$$\frac{dw}{\mathrm{W}} = \frac{\frac{dw}{dx}}{\mathrm{W}}dx + \frac{\frac{dw}{dy}}{\mathrm{W}}dy + \frac{\frac{dw}{dz}}{\mathrm{W}}dz$$

We see that the coefficients of dx, dy, dz are the same as before, and consequently we have that

$$\mathrm{U}^2 = \left(\frac{du}{dx}\right)^2 + \left(\frac{du}{dy}\right)^2 + \left(\frac{du}{dz}\right)^2$$
$$\mathrm{V}^2 = \left(\frac{dv}{dx}\right)^2 + \left(\frac{dv}{dy}\right)^2 + \left(\frac{dv}{dz}\right)^2$$
$$\mathrm{W}^2 = \left(\frac{dw}{dx}\right)^2 + \left(\frac{dw}{dy}\right)^2 + \left(\frac{dw}{dz}\right)^2$$

and the equations of condition between u, v, w, x, y, z become

$$\frac{du}{dx}\frac{dv}{dx} + \frac{du}{dy}\frac{dv}{dy} + \frac{du}{dz}\frac{dv}{dz} = 0$$
$$\frac{dv}{dx}\frac{dw}{dx} + \frac{dv}{dy}\frac{dw}{dy} + \frac{dv}{dz}\frac{dw}{dz} = 0$$
$$\frac{dw}{dx}\frac{du}{dx} + \frac{dw}{dy}\frac{du}{dy} + \frac{dw}{dz}\frac{du}{dz} = 0$$

which evidently express the orthogonality of the surfaces defined by the equations

$$u = \text{const.}\quad v = \text{const.}\quad w = \text{const.}$$

Suppose we have a function φ of x, y, z, then

$$\frac{d\varphi}{dx} = \frac{d\varphi}{du}\frac{du}{dx} + \frac{d\varphi}{dv}\frac{dv}{dx} + \frac{d\varphi}{dw}\frac{dw}{dx}$$
$$\frac{d\varphi}{dy} = \frac{d\varphi}{du}\frac{du}{dy} + \frac{d\varphi}{dv}\frac{dv}{dy} + \frac{d\varphi}{dw}\frac{dw}{dy}$$
$$\frac{d\varphi}{dz} = \frac{d\varphi}{du}\frac{du}{dz} + \frac{d\varphi}{dv}\frac{dv}{dz} + \frac{d\varphi}{dw}\frac{dw}{dz}$$

from which by aid of the previous equations

$$\left(\frac{d\varphi}{dx}\right)^2 + \left(\frac{d\varphi}{dy}\right)^2 + \left(\frac{d\varphi}{dz}\right)^2 = \mathrm{U}^2\left(\frac{d\varphi}{du}\right)^2 + \mathrm{V}^2\left(\frac{d\varphi}{dv}\right)^2 + \mathrm{W}^2\left(\frac{d\varphi}{dw}\right)^2$$

If we have two sets of rectangular axes having the same origin, the direction cosines of the one set being with reference to the other

$$\begin{matrix} \alpha & \beta & \gamma \\ \alpha_1 & \beta_1 & \gamma_1 \\ \alpha_2 & \beta_2 & \gamma_2 \end{matrix}$$

we know that we have

$$\alpha = \beta_1\gamma_2 - \beta_2\gamma_1$$
$$\beta = \gamma_1\alpha_2 - \gamma_2\alpha_1$$
$$\gamma = \alpha_1\beta_2 - \alpha_2\beta_1$$

Now the quantities $\frac{1}{\mathrm{U}}\frac{du}{dx}$ . . . are such direction cosines so we have by making

$$\frac{\mathrm{U}}{\mathrm{VW}} = \Phi_1, \frac{\mathrm{V}}{\mathrm{WU}} = \Phi_2, \frac{\mathrm{W}}{\mathrm{U_1V}} = \Phi_3$$
$$\frac{du}{dx} = \Phi_1\left(\frac{dv}{dz}\frac{dw}{dy} - \frac{dv}{dy}\frac{dw}{dz}\right)$$
$$\frac{du}{dy} = \Phi_1\left(\frac{dv}{dx}\frac{dw}{dz} - \frac{dv}{dz}\frac{dw}{dx}\right)$$
$$\frac{du}{dz} = \Phi_1\left(\frac{dv}{dy}\frac{dw}{dx} - \frac{dv}{dx}\frac{dw}{dy}\right)$$

with similar expressions for $\frac{dv}{dx}$ . . . $\frac{dw}{dz}$,

Differentiating these expressions for x,y,z respectively, and adding the results we obtain

$$\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}=\frac{d\Phi_1}{dx}\left(\frac{dv}{dz}\frac{dw}{dy}-\frac{dv}{dy}\frac{dw}{dz}\right)+\frac{d\Phi_1}{dy}\left(\frac{dv}{dx}\frac{dw}{dz}-\frac{dv}{dz}\frac{dw}{dx}\right)+\frac{d\Phi_1}{dz}\left(\frac{dv}{dy}\frac{dw}{dx}-\frac{dv}{dx}\frac{dw}{dy}\right)$$

But we have also

$$\frac{d\Phi_1}{dx}=\frac{d\Phi_1}{du}\frac{du}{dx}+\frac{d\Phi_1}{dv}\frac{dv}{dx}+\frac{d\Phi_1}{dw}\frac{dw}{dx}$$
$$\frac{d\Phi_1}{dy}=\frac{d\Phi_1}{du}\frac{du}{dy}+\frac{d\Phi_1}{dv}\frac{dv}{dy}+\frac{d\Phi_1}{dw}\frac{dw}{dy}$$
$$\frac{d\Phi_1}{dz}=\frac{d\Phi_1}{du}\frac{du}{dz}+\frac{d\Phi_1}{dv}\frac{dv}{dz}+\frac{d\Phi_1}{dw}\frac{dw}{dz}$$

consequently the above equations become

$$\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}=\frac{d\Phi_1}{du}\left\{\frac{du}{dx}\left(\frac{dv}{dz}\frac{dw}{dy}-\frac{dv}{dy}\frac{dw}{dz}\right)+\frac{du}{dy}\left(\frac{dv}{dx}\frac{dw}{dz}-\frac{dv}{dz}\frac{dw}{dy}\right)+\frac{du}{dz}\left(\frac{dv}{dy}\frac{dw}{dx}-\frac{dv}{dx}\frac{dw}{dy}\right)\right\}$$

or

$$\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}=\mathrm{UVW}\frac{d\Phi_1}{du}.$$

Similarly

$$\frac{d^2v}{dx^2}+\frac{d^2v}{dy^2}+\frac{d^2v}{dz^2}=\mathrm{U\,V\,W}\frac{d\Phi_2}{dv}$$
$$\frac{d^2w}{dx^2}+\frac{d^2w}{dy^2}+\frac{d^2w}{dz^2}=\mathrm{U\,V\,W}\frac{d\Phi_3}{dw}.$$

Differentiate the expression

$$\frac{d\varphi}{d\xi}=\frac{d\varphi}{du}\frac{du}{d\xi}+\frac{d\varphi}{dv}\frac{dv}{d\xi}+\frac{d\varphi}{dw}\frac{dw}{d\xi}$$

for ξ and then replace ξ by x, y, z successively and add the results. The sum after easy reductions becomes

$$\frac{d^2\varphi}{dx^2}+\frac{d^2\varphi}{dy^2}+\frac{d^2\varphi}{dz^2}=\mathrm{UVW}\left\{\frac{d}{du}\Phi_1\frac{d\varphi}{du}+\frac{d}{dv}\cdot\Phi_2\frac{d\varphi}{dv}+\frac{d}{dw}\cdot\Phi_3\frac{d\varphi}{dw}\right\}$$

Our equation $\Delta^2\varphi=o$ has thus become

$$\frac{d}{du}\left(\frac{\mathrm{U}}{\mathrm{VW}}\frac{d\varphi}{du}\right)+\frac{d}{dv}\left(\frac{\mathrm{V}}{\mathrm{UW}}\frac{d\varphi}{dv}\right)+\frac{d}{dw}\left(\frac{\mathrm{W}}{\mathrm{UV}}\frac{d\varphi}{dw}\right)=o.$$

It is obvious that this equation will not be altered by supposing one of the quantities U, V, W, to change its sign, hence the hypothesis that all these should be positive was in this case unnecessary.

It is not difficult to see that we have the following values for the products UVW, and $\frac{1}{\mathrm{UVW}}$: viz.

$$\mathrm{UVW}=\begin{vmatrix}\frac{du}{dx}, & \frac{du}{dy}, & \frac{du}{dz}\\ \frac{dv}{dx}, & \frac{dv}{dy}, & \frac{dv}{dz}\\ \frac{dw}{dx}, & \frac{dw}{dy}, & \frac{dw}{dz}\end{vmatrix}$$

and

$$\frac{1}{\mathrm{UVW}}=\begin{vmatrix}\frac{dx}{du}, & \frac{dy}{du}, & \frac{dz}{du}\\ \frac{dx}{dv}, & \frac{dy}{dv}, & \frac{dz}{dv}\\ \frac{dx}{dw}, & \frac{dy}{dw}, & \frac{dz}{dw}\end{vmatrix}$$

Two other methods of transforming the equations $\Delta^2\varphi=o$ are given in Todhunter's treatise on Laplace's Functions, &c. One of the processes is due to Jacobi, the transformation being effected by the aid of the calculus of variations. The other method is due to Dirichlet, and is deduced from considerations of a special case of Greene's theorum already referred to in this article.

We will now take up the case when our curvilinear co-ordinates are the so called elliptic co-ordinates. The reader is supposed to be acquainted with the properties of confocal surfaces, so we will spend no time in defining them. Let the equations of the confocal system be

$$\frac{x^2}{a^2+u}+\frac{y^2}{b^2+u}+\frac{z^2}{c^2+u}=1$$
$$\frac{x^2}{a^2+v}+\frac{y^2}{b^2+v}+\frac{z^2}{c^2+v}=1$$
$$\frac{x^2}{a^2+w}+\frac{y^2}{b^2+w}+\frac{z^2}{c^2+w}=1$$

When a^2+u, b^2+u, c^2+u are the squares on the semi-axes of the confocal ellipsoid passing through the point x, y, z.

a^2+v, b^2+v, c^2+v are the squares on the semi-axis of the confocal hyperboloid of one nappe.

a^2+w, b^2+w, c^2+w the squares on the semi-axes of the confocal hyperboloid of two nappes.

If a^2 be the greatest, and c^2 the least, of the quantities a^2, b^2, c^2, we will have

$$\begin{aligned} +\infty &> u > -c^2 \\ -c^2 &> v > -b^2 \\ -b^2 &> w > -a^2 \end{aligned}$$

Suppose now that we have the functions α, β, γ, of x, y, z, satisfying the equations

$$\Delta^2\alpha = 0 \quad \Delta^2\beta = 0 \quad \Delta^2\gamma = 0$$

and that the surfaces represented by the equations

$$\alpha = \text{const.}, \quad \beta = \text{const.}, \quad \gamma = \text{const.},$$

are orthogonal, and in consequence satisfying the equations

$$\begin{aligned} \frac{d\beta}{dx}\frac{d\gamma}{dx} + \frac{d\beta}{dy}\frac{d\gamma}{dy} + \frac{d\beta}{dz}\frac{d\gamma}{dz} &= 0 \\ \frac{d\gamma}{dx}\frac{d\alpha}{dx} + \frac{d\gamma}{dy}\frac{d\alpha}{dy} + \frac{d\gamma}{dz}\frac{d\alpha}{dz} &= 0 \\ \frac{d\alpha}{dx}\frac{d\beta}{dx} + \frac{d\alpha}{dy}\frac{d\beta}{dy} + \frac{d\alpha}{dz}\frac{d\beta}{dz} &= 0 \end{aligned}$$

now we have

$$\frac{d\varphi}{dx} = \frac{d\varphi}{d\alpha}\frac{d\alpha}{dx} + \frac{d\varphi}{d\beta}\frac{d\beta}{dx} + \frac{d\varphi}{d\gamma}\frac{d\gamma}{dx}$$

and then

$$\begin{aligned} \frac{d^2\varphi}{dx^2} = {} & \frac{d^2\varphi}{d\alpha^2}\left(\frac{d\alpha}{dx}\right)^2 + \frac{d^2\varphi}{d\beta^2}\left(\frac{d\varphi}{d\beta}\right)^2 + \frac{d^2\varphi}{d\gamma^2}\left(\frac{d\varphi}{d\gamma}\right)^2 \\ & + 2\frac{d^2\varphi}{d\beta d\gamma}\frac{d\beta}{dx}\frac{d\gamma}{dx} + 2\frac{d^2\varphi}{d\gamma d\alpha}\frac{d\gamma}{dx}\frac{d\alpha}{dx} \\ & + 2\frac{d^2\varphi}{d\alpha d\beta}\frac{d\alpha}{dx}\frac{d\beta}{dx} + \frac{d\varphi}{d\alpha}\frac{d^2\alpha}{dx^2} + \frac{d\varphi}{d\beta}\frac{d^2\beta}{dx^2} \\ & + \frac{d\varphi}{d\gamma}\frac{d^2\gamma}{dx^2} \end{aligned}$$

on forming the expressions for $\frac{d^2\varphi}{dy^2}$, $\frac{d^2\varphi}{dz^2}$ and adding the results we will have by virtue of the above equations,

$$\begin{aligned} \Delta^2\varphi = {} & \frac{d^2\varphi}{d\alpha^2}\left\{\left(\frac{d\alpha}{dx}\right)^2 + \left(\frac{d\alpha}{dy}\right)^2 + \left(\frac{d\alpha}{dz}\right)^2\right\} \\ & + \frac{d^2\varphi}{d\beta^2}\left\{\left(\frac{d\beta}{dx}\right)^2 + \left(\frac{d\beta}{dy}\right)^2 + \left(\frac{d\beta}{dz}\right)^2\right\} \\ & + \frac{d^2\varphi}{d\gamma^2}\left\{\left(\frac{d\gamma}{dx}\right)^2 + \left(\frac{d\gamma}{dy}\right)^2 + \left(\frac{d\gamma}{dz}\right)^2\right\} \end{aligned}$$

now let

$$\frac{d\alpha}{du} = \frac{1}{\sqrt{(a^2+u)(b^2+u)(c^2+u)}}$$

$$\frac{d\beta}{dv} = \frac{1}{\sqrt{(a^2+v)(b^2+v)(c^2+v)}}$$

$$\frac{d\gamma}{dw} = \frac{1}{\sqrt{(a^2+w)(b^2+w)(c^2+w)}}$$

The corresponding integrals are clearly

$$\alpha = \int_u^{\infty} \frac{du}{\sqrt{(a^2+u)(b^2+u)(c^2+u)}}$$

$$\beta = \int_v^{-c^2} \frac{dv}{\sqrt{(a^2+v)(b^2+v)(c^2+v)}}$$

$$\gamma = \int_w^{-b^2} \frac{dw}{\sqrt{(a^2+w)(b^2+w)(c^2+w)}}$$

now

$$\left(\frac{d\alpha}{dx}\right)^2 + \left(\frac{d\alpha}{dy}\right)^2 + \left(\frac{d\alpha}{dz}\right)^2 = \frac{1}{(a^2+u)(b^2+u)(c^2+u)} \left\{\left(\frac{du}{dx}\right)^2 + \left(\frac{du}{dy}\right)^2 + \left(\frac{du}{dz}\right)^2\right\}$$

since $\frac{d\alpha}{dx} = \frac{d\alpha}{du}\frac{du}{dx}$.

Further, since

$$\frac{x^2}{a^2+u} + \frac{y^2}{b^2+u} + \frac{z^2}{c^2+u} = 1$$

we have

$$\frac{du}{dx}\left\{\frac{x^2}{(a^2+u)^2} + \frac{y^2}{(b^2+u)^2} + \frac{z^2}{(c^2+u)^2}\right\} = \frac{2x}{a^2+u}$$

with similar expressions for $\frac{du}{dy}$, $\frac{du}{dz}$. Hence, squaring, adding and dividing through by $\frac{x^2}{(a^2+u)^2} + \frac{y}{(b^2+u)^2} + \frac{z^2}{(c^2+u)^2}$ and we have

$$\left\{\frac{x^2}{(a^2+u)^2} + \frac{y^2}{(b^2+u)^2} + \frac{c^2}{(c^2+u)^2}\right\} \left\{\left(\frac{du}{dx}\right)^2 + \left(\frac{du}{dy}\right)^2 + \left(\frac{du}{dz}\right)^2\right\} = 4$$

But from the equations of the system of confocal surfaces we can deduce

$$1 - \left\{\frac{x^2}{a^2+w} + \frac{d^2}{b^2+w} + \frac{z^2}{c^2+w}\right\} = \frac{(\omega-u)(\omega-v)(\omega-w)}{(\omega+a^2)(\omega+b^2)(\omega+c^2)}$$

when ω is any quantity whatever. For this expression is of the zero dimensions in ω, u, v, w, it vanishes when $\omega = u, v$, or w, and for these values of ω only it becomes infinite for $\omega = -a^2, -b^2$, or $-c^2$, and for these values of ω only, and it is $=1$ for $\omega = \infty$. From this, multiplying

successively by $a^2+\omega$, $b^2+\omega$, $c^2+\omega$, and then putting $\omega=-a^2,-b^2,-c^2$,

$$x^2=\frac{(a^2+u)(a^2+v)(a^2+w)}{(a^2-b^2)(a^2-c^2)}$$
$$y^2=\frac{(b^2+u)(b^2+v)(b^2+w)}{(b^2-c^2)(b^2-a^2)}$$
$$z^2=\frac{(c^2+u)(c^2+v)(c^2+w)}{(c^2-a^2)(c^2-b^2)}$$

again, differentiating with respect to ω and then putting $\omega=u$,

$$\frac{x^2}{(a^2+u)^2}+\frac{y^2}{(b^2+u)^2}+\frac{z^2}{(c^2+u)^2}=\frac{(u-v)(u-w)}{(a^2+u)(b^2+u)(c^2+u)}$$

also

$$\frac{x^2}{(a^2+v)^2}+\frac{y^2}{(b^2+v)^2}+\frac{z^2}{(c^2+v)^2}=\frac{(v-w)(v-u)}{(a^2+v)(b^2+v)(c^2+v)}$$

$$\frac{x^2}{(a^2+w)^2}+\frac{y^2}{(b^2+w)^2}+\frac{z}{(c^2+w)^2}=\frac{(w-u)(w-v)}{(a^2+w)(b^2+w)(c^2+w)}$$

$\therefore$

$$\left(\frac{du}{dx}\right)^2+\left(\frac{du}{dy}\right)^2+\left(\frac{du}{dz}\right)^2=4\frac{(a^2+u)(b^2+u)(c^2+u)}{(u-v)(u-w)}$$

$$\left(\frac{dv}{dx}\right)^2+\left(\frac{dv}{dy}\right)^2+\left(\frac{dv}{dz}\right)^2=4\frac{(a^2+v)(b^2+v)(c^2+v)}{(v-w)(v-u)}$$

$$\left(\frac{dw}{dx}\right)^2+\left(\frac{dw}{dy}\right)^2+\left(\frac{dw}{dz}\right)^2=4\frac{(a^2+w)\,(b^2+w)\,(c^2+w)}{(w-u)(w-v)}$$

These three quantities are the functions defined by the U^2, V^2, W^2, of the preceding part of the chapter. By virtue of these we have

$$\left(\frac{d\alpha}{dx}\right)^2+\left(\frac{d\alpha}{dy}\right)^2+\left(\frac{d\alpha}{dz}\right)^2=\frac{4}{(u-v)(u-w)}$$
$$\left(\frac{d\beta}{dx}\right)^2+\left(\frac{d\beta}{dy}\right)^2+\left(\frac{d\beta}{dz}\right)^2=-\frac{4}{(v-w)(v-u)}$$
$$\left(\frac{d\gamma}{dx}\right)^2+\left(\frac{d\gamma}{dy}\right)^2+\left(\frac{d\gamma}{dz}\right)^2=\frac{4}{(w-u)(w-v)}$$

The negative sign in the second being due to the fact that β is imaginary. We have thus

$$\Delta^2\varphi=\frac{4}{(v-w)(w-u)(u-v)}\left\{(w-v)\frac{d^2\varphi}{d\alpha^2}+(u-w)\frac{d^2\varphi}{d\beta^2}+(v-u)\frac{d^2\varphi}{d\gamma^2}\right\}$$

and consequently $\Delta^2\varphi=o$ becomes

$$(w-v)\frac{d^2\varphi}{d\alpha^2}+(u-w)\frac{d^2\varphi}{d\beta^2}+(v-u)\frac{d^2\varphi}{d\gamma^2}=o$$

or

$$(w-v)\left\{[(a^2+u)(b^2+u)(c^2+u)]^{\frac{1}{2}}\frac{d}{du}\right\}^2\varphi+(u-w)\left\{[(a^2+v)(b^2+v)(c^2+v)]^{\frac{1}{2}}\frac{d}{dv}\right\}^2\varphi+(v-u)\left\{[(a^2+w)(b^2+w)(c^2+w)]^{\frac{1}{2}}\frac{d}{dw}\right\}^2\varphi=o$$

We have also

$$\left(\frac{d\varphi}{dx}\right)^2+\left(\frac{d\varphi}{dy}\right)^2+\left(\frac{d\varphi}{dz}\right)^2=\frac{4}{(u-v)(u-w)}\left(\frac{d\varphi}{da}\right)^2-\frac{4}{(v-w)(v-u)}\left(\frac{d\varphi}{d\beta}\right)^2+\frac{4}{(w-u)(w-v)}\left(\frac{d\varphi}{d\gamma}\right)^2.$$

We saw that the parameters u, v, w were confined between the following limits:

u between $-c^2$ and $+\infty$
v " $-b^2$ " $-c^2$
w " $-a^2$ " $-b^2$

so that by introducing the three angles θ, Φ, ψ, we may express these quantities by means of the following equations:

$$u=\frac{a^2\cos^2\theta-c^2}{\sin^2\theta}$$

$u=-c^2$ for $\theta=\frac{\pi}{2}$
$u=\infty$ " $\theta=o$

$v=-b^2\cos^2\Phi-c^2\sin^2\Phi$ $v=-c^2$ for $\Phi=o$
$v=-b^2$ " $\Phi=\frac{\pi}{2}$

$w=-b^2\sin^2\psi-a^2\cos^2\psi$ $w=-a^2$ " $\psi=o$
$w=-b^2$ " $\psi=\frac{\pi}{2}$

from these we have

$$du=2(c^2-a^2)\cot\theta\cos^2\theta d\theta$$
$$dv=2(c^2-b^2)\sin\Phi\cos\Phi\,d\Phi$$
$$dw=2(a^2-b^2)\sin\psi\cos\psi\,d\psi$$

These substituted in the expressions for α, β, γ, give for these quantities

$$\alpha=-\frac{2}{\sqrt{a^2-c^2}}\int_o^\theta\frac{d\theta}{\sqrt{1-k^2\sin^2\theta}}$$

$$= -\frac{2}{\sqrt{a^2-c^2}}\mathrm{F}(k,\theta)$$

$$\beta = \frac{i\,2}{\sqrt{a^2-c^2}}\int_o^{\Phi}\frac{d\Phi}{\sqrt{1-k'^2\cos^2\Phi}}$$

$$= \frac{2i}{\sqrt{a^2-c^2}}[\mathrm{F}(k')-\mathrm{F}(k,'\Phi)]$$

$$\gamma = \frac{2}{\sqrt{a^2-c^2}}\int_o^{\psi}\frac{d\psi}{\sqrt{1-k^2\sin^2\psi}}$$

$$= \frac{2}{\sqrt{a^2-c^2}}\mathrm{F}(k,\psi)$$

when $\mathrm{F}(k')$ denotes the complete elliptic integral of the first kind with modulus k', and $k^2=\frac{a^2-b^2}{a^2-c^2}$. From the equations of our confocal surfaces we have the following particular cases:

for $u=\infty$ the surface represents an infinitely great sphere whose radius $=\sqrt{u}$.

If u differ from $-c^2$ by an indefinitely small positive quantity we have for the surface

$$z=o,\ \frac{x^2}{a^2-c^2}+\frac{y^2}{b^2-c^2}<1$$

that is the part of the plane xy lying within the focal ellipse.

If v differ from $-c^2$ by an indefinitely small negative quantity the surface will be given by

$$z=o,\ \frac{x^2}{a^2-c^2}+\frac{y^2}{b^2-c^2}>1$$

the part of the plane of xy lying without the focal ellipse.

And similarly if v differs from $-b^2$ by a positive infinitesimal we have for the surface

$$y=o,\ \frac{x^2}{a^2-b^2}-\frac{z^2}{b^2-c^2}<1$$

the portion of the plane of xz lying within the focal hyperbola.

If v differ from $-b^2$ by an indefinitely small negative quantity we get

$$y=o,\ \frac{x^2}{a^2-b^2}-\frac{z^2}{b^2-c^2}>1$$

the portion of the plane xz lying without the focal hyperbola.

Finally if v differs from $-a^2$ by an indefinitely small positive quantity we have simply

$$x=o,$$

or the surface is in this case the whole plane of yz.

We see now that for $u=v=-c^2$ that we have the focal ellipse

$$z=o,\ \frac{x^2}{a^2-c^2}+\frac{y^2}{b^2-c^2}=1$$

and for $v=v=-b^2$ the focal hyperbola

$$y=o,\ \frac{x^2}{a^2-b^2}-\frac{y^2}{b^2-c^2}=1$$

The same results can be obtained by discussing the values of α, β, γ, (Max. Elec. & Mag) but the manner adopted is the simpler.

We will resume now our integrals

$$\alpha = -\frac{2}{\sqrt{a^2-c^2}}\int_o^{\theta}\frac{d\theta}{\sqrt{1-k^2\sin^2\theta}}$$

$$\beta = \frac{2}{i\sqrt{a^2-c^2}}\int_o^{\Phi}\frac{d\Phi}{\sqrt{1-k'^2\cos^2\Phi}}$$

$$\gamma = \frac{2}{\sqrt{a^2-c^2}}\int_o^{\psi}\frac{d\psi}{\sqrt{1-k^2\cos^2\psi}}$$

and examine them in the cases when our surfaces are of revolution. Suppose first $\alpha=b$, then since $k=\frac{a^2-b^2}{a^2-c^2}$, we have $k=o$ and $k'=1$, giving

$$\alpha = -\frac{2\theta}{\sqrt{a^2-c^2}}$$

$$\beta = \frac{2\log\tan\frac{\Phi}{2}}{i\sqrt{a^2-c^2}}$$

$$\gamma = \frac{2\psi}{\sqrt{a^2-c^2}}$$

From these equations we have the following:

$$\sin\theta = -\sin\frac{\sqrt{a^2-c^2}}{2}\alpha$$

$$\cos\theta = \cos\frac{\sqrt{a^2-c^2}}{2}\alpha$$

from these we have for u

$$u = \frac{a^2\cos^2\theta-c^2}{\sin^2\theta} = \frac{a^2\cos^2\frac{\sqrt{a^2-c^2}}{2}\alpha-c^2}{\sin^2\frac{\sqrt{a^2-c^2}}{2}\alpha}$$

$$= a^2\cot^2\lambda\alpha - c^2\mathrm{cosec}_2{}^2\lambda\alpha$$

designating by λ the quantity $\frac{\sqrt{a^2-c^2}}{2}$.

$$v=-a^2\cos^2\Phi-c^2\sin^2\Phi$$

$$=-\left\{\frac{a^2\left\{\begin{matrix}i\lambda\beta-i\lambda\beta\\ \varepsilon\quad -\quad \varepsilon\end{matrix}\right\}^2+4c^2}{\left\{\begin{matrix}i\lambda\beta\quad -i\lambda\beta\\ \varepsilon\quad +\quad \varepsilon\end{matrix}\right\}^2}\right\}$$

$$w=-a^2$$

We will here use for brevity a nomenclature used by Maxwell in the same connection. If we call the exponential quantity $\frac{1}{2}\begin{pmatrix}i\lambda\beta & -i\lambda\beta\\ \varepsilon & +\ \varepsilon\end{pmatrix}$ the hyperbolic cosine of $i\lambda\beta$—or $\cos h\, i\lambda\beta$ and also call $\frac{1}{2}\begin{pmatrix}i\lambda\beta & -i\lambda\beta\\ \varepsilon & -\ \varepsilon\end{pmatrix}$ the hyposine of $i\lambda\beta$ or $\sin h\, i\lambda\beta$. We will now have for our surfaces

$$\frac{x^2+y^2}{\operatorname{cosec}^2\lambda a}+\frac{z^2}{\cot^2\lambda a}=u^2-c^2$$

$$\frac{x^2+y^2}{\operatorname{cosech}^2 i\lambda\beta}-\frac{z^2}{\coth^2\lambda\beta}=u^2-c^2$$

$$\frac{x^2}{\cos^2\lambda\gamma}-\frac{y^2}{\sin^2\lambda\gamma}=o$$

Now suppose that we have $b=c$, then $k=1$ and $k'=0$, giving us

$$\alpha=-\frac{1}{\lambda}\int_o^\theta\frac{d\theta}{\cos\theta}=-\frac{1}{\lambda}\log\tan\frac{\pi-2\theta}{4}$$

$$\beta=\frac{\Phi}{i\lambda}$$

$$\gamma=\frac{1}{\lambda}\int\frac{d\psi}{\cos\psi}=\frac{1}{\lambda}\log\tan\frac{\pi-2\psi}{4}$$

Thus we have the ellipsoids

$$\frac{x^2}{\tanh^2 i\lambda a}+\frac{y^2+z^2}{\operatorname{sech}^2 i\lambda a}=a^2-c^2$$

the meridional planes

$$\frac{x^2}{\cos^2\lambda\beta}-\frac{z^2}{\sin^2\lambda\beta}=o$$

and the hyperboloids of two nappes

$$\frac{x^2}{\coth^2\lambda\gamma}-\frac{y^2+z^2}{\operatorname{cosech}^2\lambda\gamma}=a^2-c^2$$

In closing this chapter we will just observe the forms of the potential of the ellipsoid and of the indefinitely long circular cylinder. We will designate the potential of the ellipsoid by φ at any point x, y, z, remembering that φ and its derivatives with respect to x, y and z, are continuous single valued functions, and that they all vanish at infinity. The ellipsoid is given by the equation

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1$$

assuming as usual the density to be equal to unity. We know that if the point x,y,z lies without the ellipsoid, that we have

$$\Delta^2\varphi=o$$

and if it lies within

$$\Delta^2\varphi=-4\pi.$$

Denote by q the greatest root of the equation

$$\frac{x^2}{a^2+q}+\frac{y^2}{b^2+q}+\frac{z^2}{c^2+q}=1$$

at the surface of the ellipsoid $q=o$. We have now for exterior points

$$\varphi=\pi abc\int_q^\infty\frac{1-\frac{x^2}{a^2+\chi}-\frac{y^2}{b^2+\chi}-\frac{z^2}{c^2+\chi}}{\sqrt{(a^2+\chi)(b^2+\chi)(c^2+\chi)}}d\chi$$

and for interior points

$$\varphi=\pi\,a\,b\,c\int_o^\infty\frac{1-\frac{x^2}{a^2+\chi}-\frac{y^2}{b^2+\chi}-\frac{z^2}{c^2+\chi}}{\sqrt{(a^2+\chi)(b^2+\chi)(c^2+\chi)}}d\chi$$

The similarity will be noted between these expressions and those for the fractions α, β and γ. By forming the derivatives of φ with respect to x, y and z, it will be seen that there is no discontinuity at the surface of the ellipsoid or for $q=o$. We will also find for exterior points

$$\frac{d^2\varphi}{dx^2}=2\pi abc\left\{\frac{\frac{x}{a^2+q}\frac{dq}{dx}}{\sqrt{(a^2+q)(b^2+q)(c^2+q)}}-\int_q^\infty\frac{d\chi}{(a^2+\chi)\sqrt{a^2+\chi)(b^2+\chi)(c^2+\chi}}\right\}$$

and for interior points

$$\frac{d^2\varphi}{dx^2}=-2\pi abc\int_o^\infty\frac{d\chi}{(a^2+\chi)\sqrt{(a^2+\chi)(b^2+\chi)(c^2+\chi)}}$$

By simply advancing the letters we get the corresponding second derivatives for

y and z. We know from a former equation that

$$\frac{dx}{dq}\left\{\frac{x^2}{(a^2+q)^2}+\frac{y^2}{(b^2+q)^2}+\frac{z^2}{(c^2+q)^2}\right\}=\frac{2x}{a^2+q}$$

writing the corresponding expressions for yard z, multiplying respectively by $\frac{dq}{dx}, \frac{dq}{dy}, \frac{dq}{dz}$ and adding, and we obtain

$$\frac{x}{a^2+q}\frac{dq}{dx}+\frac{y}{b^2+q}\frac{dq}{dy}+\frac{z}{c^2+q}\frac{dq}{dz}=2$$

We have also

$$\int\left(\frac{1}{a^2+x}+\frac{1}{b^2+x}+\frac{1}{c^2+x}\right)\frac{d\chi}{\sqrt{(a^2+\chi)(b^2+\chi)(c^2+\chi)}}=\frac{2}{\sqrt{(a^2+\chi)(b^2+\chi)(c^2+\chi)}}$$

So forming the expression for $\Delta^2\varphi$ we find that for internal points we have

$$\Delta^2\varphi=-4\pi$$

and for external points

$$\Delta^2\varphi=o$$

so that these forms of φ satisfy all the necessary conditions.

Now if $c=\infty$ we will obviously have a cylinder whose axis is the axis of z, and whose intersection with the plane xy is

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$$

let the bases of this cylinder be $z=\pm l$ when l is infinitely greater than any values that may be assigned to a and b. Denote an element of the ellipse by ds, the z ordinate of any point by λ then we have for φ

$$\varphi=\int_{-l}^{l}\int\frac{ds d\lambda}{\sqrt{\rho^2+(\lambda-z)^2}}$$

when ρ^2 = the distance from the element $ds d\lambda$ to the point (x, y, z). This integral is readily found to be

$$\varphi=-2\int ds(\log\rho-\log zl)$$

With reference to the first part of this integral, viz. $-2\int ds \log\rho$ we may see representing it by U that for a point within the surface of the cylinder that we have

$$\frac{d^2U}{dx^2}+\frac{d^2U}{dy^2}=-4\pi$$

and for an external point

$$\frac{d^2U}{dx^2}+\frac{d^2U}{dy^2}=o$$

We may write our equation for φ after the next integration

$$\varphi=U+2\pi\, ab\, 2l$$

Our equations for U are satisfied by

$$U=\pi ab\left(\int_o^{q'}\frac{1-\frac{x^2}{a^2+\chi}-\frac{y^2}{b^2+\chi}}{\sqrt{(a^2+\chi)(b^2+\chi)}}d\chi+1-\log q'\right)$$

for an internal point

$$U=\pi ab\left(\int_q^{q'}\frac{1-\frac{x^2}{a^2+\chi}-\frac{y^2}{b^2+\chi}}{\sqrt{(a^2+\chi)(b^2+\chi)}}d\chi+1-\log q'\right)$$

when q' is an indefinitely great constant and as before q is the positive root of the equation

$$\frac{x^2}{a^2+q}+\frac{y^2}{b^2+q}=1$$

By integration we have for internal points

$$U=\pi ab\left(2\log\frac{2}{a+b}+1\right)-2\pi\frac{bx^2+ay^2}{a+b}$$

for $a=b$ the ellipse becomes a circle and we have

$$U=\pi a^2(1-2\log a)-\pi\rho^2, \text{ when } \rho a=xa+y^2$$

and for an external point in this case

$$U=-2\pi a^2\log\rho.$$

The Shannon is in difficulties again. Her boilers are rapidly decaying, and it is expected will have to be replaced by new ones, although she has been but little over twelve months in commission. Milk of lime has been used, and to neutralize the galvanic action ingots of zinc have been suspended in various parts of them, but with little effect. Her engines are also said to be working very unsatisfactorily.

HYDROLOGY OF THE MISSISSIPPI.

A REPLY TO GEN. ABBOT.

By JAMES B. EADS, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

My attention has recently been called to an article by Gen. Henry L. Abbot, U. S. Engineer, in the January number of your Magazine, reviewing my review of Humphrey and Abbot's report on the Physics and Hydraulics of the Mississippi River, published in your Magazine last September. As I am charged by Gen. Abbot with making reckless misstatements in my review, I beg space in your columns to reply to this grave accusation.

Gen. Abbot says: "As an example of the reckless misstatements of facts which characterize the whole review, I quote the following paragraph:

"'By reference to pages 135 and 137 [Mississippi Report] it will be seen that this extract contains an astonishing exaggeration. Instead of *three years*, the current and sediment observations only occupied *eight months* at Columbus and one year at Carrollton.

"'When we remember that the junior author of the report on the Mississippi River was a prominent member of the Levee Commission, and that the senior author, as Chief of Engineers, warmly endorsed its report, it is difficult to reconcile this careless statement with the unusual scientific exactness which required four decimals to record their measurements of the current.'

"Turning to the page next preceding the one mentioned, we find a table giving in detail the results of the *two* years of sediment observations made by a party of the survey of Carrollton; and on page 142 another giving the details of a continuous series of nearly three months' observations, made by Prof. Riddell. These, with the eight months' record at Columbus, sufficiently sustain the accuracy of our language.

"After such an exposure it is sufficient, without devoting space to other instances of similar inaccuracy in this review, to ask the reader to apply the old maxim '*ex uno disce omnes*,' and take the precaution to refer to the report itself before accepting as trustworthy Capt. Eads' statements respecting its contents."

Gen. Abbot in all this has misled his readers in a manner that is simply unpardonable. A few words of explanation will enable them to appreciate this fact.

The quotation containing the astonishing exaggeration, to which I referred, is not repeated by Gen. Abbot. It is as follows: * * * "the assumption that the river water is always charged with sediment to its maximum supporting capacity, * * * has been shown by three years of accurate daily observations at Carrollton and Columbus, to be utterly unfounded. Indeed, it often happened that the amount of sedimentary matter per cubic foot of water was greater in low than in high stages of the river, and never was there any fixed relation between these quantities. In other words, Mississippi river water is undercharged with earthy matter, and therefore no reasonable reduction of its flood velocity by an outlet will produce a deposit in the bed below."

This extract is taken from the report of the United States Levee Commission of 1874–5 (see Executive Document, 127 H. R., 43d Congress, 2d session, page 8). Gen. Abbot was a prominent member of the Commission, and its report was warmly endorsed by Gen. Humphreys. It was to their observations at Columbus and Carrollton that the commission refers in the extract, as establishing the theory advanced by them in their report in 1861, namely, that no relation whatever exists between the velocity of current and the quantity of earthy matter carried in suspension by the river water.

The important bearing which the truth or falsity of this theory has upon the question of river improvement can scarcely be over-estimated. But as this is fully shown in my review it is not necessary to discuss it here. It is sufficient to say that I had only a short time before the publication of the Commissioner's report, declared the very converse

of this theory to be true, namely, that an intimate relation *does* exist between the velocity of current and the quantity of sediment suspended in the water of all rivers flowing through alluvial deposits, and that if the current be reduced, a portion of the sediment will be deposited in consequence. Gen. Abbot flatly declares that this assumption has been shown to be utterly misfounded by *three years* of daily observations at Columbus and Carrollton; and when I declared this three years' statement to be an exaggeration, instead of admitting his error frankly, when pointed out, he strives to establish its truth by a deceptive reference to irrelevant observations made on sediment alone, to determine another question altogether, and which observations were accompanied by no measurements of the current whatever. If any one feels sufficient interest in the issue of veracity which General Abbot has raised, he has only to turn to the parts of Humphreys' and Abbot's report to which he refers, and he will there find that the second year's experiments at Carrollton consisted in simply taking one sample of water from the surface daily, and weighing the sediment contained in it,* and that these experiments were not accompanied by any current observations whatever, consequently they have no bearing at all upon the question referred to by the Levee Commission. He will see that the results of the second year's experiments were only used by Humphreys and Abbot as a means of approximately determining the amount of sediment in the water of the river, and of estimating the quantity of alluvion annually discharged into the sea.†

The first year's observations are referred to in the text as "elaborate measurement" (see foot note) and were accompanied by current measurements for one year, which are shown in Appendix "D." General Abbot says in his reply: "Our analysis of these observations, stated in brief, consisted in plotting two curves, of which the common abscissas were times; and the ordinates, respectively *the mean velocity of the river in feet per second*, and the corresponding number of grains of earthy matter held in suspension by one cubic foot, representing the average for the whole river."

It is an insult to the intelligence of his readers to tell them that observations made on sediment alone, during the second year at Carrollton, could be brought into such an analysis as this.

If the reader will follow General Abbot's suggestion and take the precaution to refer to the report itself, he will see that Professor Riddell's experiments were made on sediment alone also; and that they could not have been in the mind of General Abbot, when he made his extravagant statement about the daily labor of three years at Columbus and Carrollton, for the reasons: *First*, that they were not made at either of these places. *Second*, because they were not made "daily," but, as Professor Riddell says, "at intervals of three days." *Third*, because they were made many years before those at Carrollton and Columbus were ever heard of; and *finally*, as they occupied but fourteen days in 1843 and but thirty-five days in 1846, it evidently required a mind given to romancing rather than to science, to justify General Abbot in what may be politely termed a poetic license, when he speaks of them as "*nearly three months*' observations made by Professor Riddell." ‡

* "These elaborate measurements were begun on February 17, 1851, and continued fifty-two weeks. During the next year it was not deemed necessary to make the operation so laborious, since the ratio between the sediment contained in the water at any one of the positions, and that contained in the whole river might fairly be considered to be determined by the first year's observations. For the second year, therefore, only one sample daily was obtained. It was taken from the surface at the position 300 feet from the east bank."

See H. and A.'s report, page 133. (N.B.—The numbers of the pages in the old edition are somewhat different from the new one, to which reference is here made.)

† "The observations of the second year show what caution should be observed in attempting to generalize upon the proportion of sediment contained in the Mississippi water, even when the observations extend over long periods. If it be allowable to assume the same ratio to exist as in 1851-52, between the amount of sediment in the entire river and that at the surface of the first division, we have for the maximum, minimum, and mean proportions of sediment of water by weight during the second year—the fractions $\frac{1}{5\,7\,2}$ (fourth week of April) $\frac{1}{3\,5\,6\,4}$ (third week of November), and $\frac{1}{1\,4\,1\,6}$, which differ materially from the above values for the previous year."

"Before drawing any general conclusion therefore as to the amount of sedimentary matter annually discharged by the Mississippi into the Gulf, it is well to examine all other data upon the subject. The observations of this survey at Columbus in 1858 are the first in order.

"These observations were undertaken voluntarily by Mr. Fillebrown's assistant, Mr. Webster, and continued till he left the party in June; from that date they were made by Mr. Fillebrown." See H. and A.'s report, page 135.

‡ "Professor Riddell's first experiments upon the amount of sediment contained in Mississippi water are reported in the letter addressed to Sir Charles Lyell on March 5, 1846." * * * * * * * * * * * * "Professor Riddell's second experiments were made when a member of a committee appointed by the Association of American Geologists and

In view of this record, does it become General Abbot to charge me, or any one else, with reckless misstatements?

I believe that all true friends of science will agree that to exaggerate the proofs advanced in favor of any particular theory, one may be supporting, is wholly inexcusable; as it tends to embarrass scientific investigation and delay the development of truth. The careless way in which the assertion in question was uttered is bad enough, for it leads the reader to infer that these experiments occupied three years at each place, instead of but eight months at one, and but twelve months at the other. It is, however, an extravagant statement, even when construed in the most liberal manner. Still, the Levee Commission should not, I think, be held responsible for it, as the undue dilation was evidently prepared by General Abbot himself as a part of the Commission's report, and as it referred to his own labors it would have been a discourtesy in the other members to have questioned its correctness. At all events his sole title to the distinguished honor of trying to sustain a gross misstatement of fact, after its extravagance was exposed, will not hereafter be questioned or coveted by others. General Abbot declares that my criticism of their views respecting the bed of the river is unfair, as it infers that their opinions of its character are based solely on their soundings in the river itself.

On page 16 of the Levee Commission's report, there occurs this plain and positive statement: "Very numerous soundings, with leads adapted to bring up samples of the bottom, were made by the Mississippi Delta Survey throughout the whole region between Cairo and the Gulf. They showed conclusively that the *real bed*, upon which rests the shifting sand bars and mud banks made by local causes, is always found in a stratum of hard blue clay, quite unlike the present deposits of the river."

I am at a loss to perceive the unfairness of confining my investigation to the *facts* referred to, as *conclusively showing the character of the real bed of the river.*

The statement declares its real character is conclusively proved by the soundings, and if this be so, there is no necessity of looking for any other evidence of the fact. Certainly the proper place to look for this was in Humphreys' and Abbot's report. There, on page 90, the reader is told that the result of these soundings are exhibited in appendix "C." I, accordingly, looked there, expecting, of course, to find a record of very numerous samples of this hard blue clay. My surprise can be better imagined than described, when I assure the reader that I found appendix "C" actually contains, as the result of the "very numerous soundings" referred to, only thirty-five (35) samples, in a stretch of river six hundred and fifty miles long between Cape Girardeau, Mo., and Vicksburg, Miss., and of these thirty-five, *only seven were of clay alone!* Twenty-five of them being of pure sand, had not even a trace in them of the *real bed*—the ancient geologic blue clay of Humphreys' and Abbot's—the clay which they assure us is "quite unlike the present deposits of the river."

As an evidence that I am unjustly charged with unfairness, I can assure the reader that I did not come to the conclusion that this statement about *very numerous* samples of this hard blue clay was slightly exaggerated, until I had searched through the 456 quarto pages of the report, and through all of its appendices from "A" to "M" inclusive, to see if the remainder of these very numerous sounding were not recorded somewhere else than in appendix "C." I was encouraged in this search by the assurance on page 90, that "A knowledge of the character of the bed of the Mississippi river is of the highest practical importance, as will be hereafter seen, and great efforts have been made to acquire it," and also by the declaration on page 14, that "great pains were accordingly taken to collect specimens of the bed wherever soundings were made." Failing completely in my search to find other evidence that very numerous soundings had really been made to ascertain the character of the bed, I concluded that their "great pains" and "great efforts" in this direction had proved a melancholy failure. I think I shall not be alone in the belief, when others examine the report for themselves, that it would have

Naturalists to ascertain the amount of sediment carried into the sea by the Mississippi river. His report was read at the meeting of this body in 1846." See page 141, H. and A.'s report.

been an astonishing exaggeration if General Abbot had referred to these soundings as even *numerous*, without positively declaring that this important question had been *conclusively* settled by *very* numerous ones made by the Delta Survey.

General Abbot challenges my statement in reference to General Humphreys' recommendation to Congress that a canal should be cut around the east end of the bridge at St. Louis, with a drawbridge over it, because the steamboat chimneys could not be lowered down to allow them to pass under the arches of the bridge. He says: "From his statement above quoted, it would be inferred that engineer officers had never heard that steamboat chimneys could be lowered, and that as soon as the knowledge that such a thing was possible reached the ears of Congress, it at once decided not to interfere with the bridge."

One would think, to read all he says on this subject, that the Bridge Board and the chief of engineers, *at that time*, thought it was child's play to raise and lower these chimneys. This is what the Board says in its first report on the subject:

"Although it is a comparatively easy task to lower small chimneys, dealing with those of a large size is a very serious matter indeed. Their weight is so utterly disproportionate to their strength, even when new, that no machinery yet devised will enable large chimneys to be lowered, either wholly or in part, without very great labor and danger."*

Being anxious to save the bridge from mutilation, I reviewed the report and stated among other things that it was entirely feasible to raise and lower the largest chimneys used on the river, and that the cost of the hinges and necessary appliances to enable the boat to do this would not exceed one thousand or fifteen hundred dollars.

A second report was made after my review appeared, and in answer to my assurance above-stated, the Board made the following polite rejoinder:

"This statement may or may not be correct, but there is no proof of it other than the statement itself, and a document signed by 'Thirteen experienced steamboat captains.' As the apparatus recommended is not stated to be in use, and presumably has been invented by Mr. Eads, or some friend of his, it must be received with the usual discount due to the statements of inventors."* Comment on this is unnecessary.

General Abbot says: "Finally, *the only individual connection of the Chief of Engineers with these matters was that the reports of the Board were submitted to the Secretary of War through him*, as required by the Army Regulations, and were formally approved with the recommendation "that the matter be submitted to Congress at its next session for such action as in their judgment may seem to be necessary." These italics are his.

In its report the Board says: "The Board are, therefore, unanimously of the opinion that the bridge, as at present designed, will prove a very serious obstruction to the free navigation of the Mississippi River." Again: "They would, therefore, recommend as the most feasible modification," etc., etc. "Let a canal, or rather an open cut, be formed behind the east abutment of the bridge, giving at the abutment a clear width of waterway of 120 feet."

It should be remembered that these remarkable "views and recommendations" were made in relation to a bridge whose central arch is over ninety feet in clear height above low water, with a clear span between abutments of five hundred and twenty feet. It is the longest clear span in the world (excepting suspension bridges), yet General Humphreys says in his report: "The Board in addition, state that arched trusses, like those in the bridge under consideration 'present so many difficulties to free navigation, that in future their use should be prohibited in plans for bridges over navigable streams.'"†

General Humphreys, in his report, recites the points at issue, and the reasons for the conclusions of the Board, after which he says: "The views and recommendations of the Board are concurred in by me."

The Board, according to General Abbot, always knew that the chimneys could be easily lowered. He indignantly

*See report; Executive Doc. 194, 43d Congress, 1st Session, page 5.

* See 2d report, page 24, same document.
† See page 2, Bridge report, same document.

exclaims: "Even the little surveying steamer, the Major Sanders, *in charge of one of the members of the Board*, was provided with hinged chimneys."* He says in effect to his readers: "Don't believe the reckless misstatements of Mr. Eads; General Humphreys never did such a silly thing as to recommend a canal around the end of the St. Louis bridge. *The only individual connection of the Chief of Engineers* with that discreditable affair was in transmitting the report of the Board to the Secretary of War; army regulations required him to do this." Of course, he could not have *disapproved* of the report.

With these facts, quotations, and references, I submit the question of veracity raised by General Abbot, and leave it to the dispassionate reader to determine who has been making "reckless misstatements."

[NOTE BY THE EDITOR].

General Abbot announced in his article in our January number that he does not propose to continue this discussion. It seems, therefore, only fair to refer to certain points in this letter, where Mr. Eads has apparently misunderstood his position.

General Abbot does not state that "no relation whatever" exists between the velocity and the quantity of earthy matter held in suspension—which is manifestly absurd, since water at rest supports no earthy matter—but that on the Mississippi river observation has established that no "fixed relation" exists between these quantities, and hence that the water cannot be charged to its *maximum supporting capacity*.

With reference to the second year's observations at Carrollton, Mr. Eads overlooks the fact that the *stand of the river* corresponding to the sediment observations is given (page 525 and plate IX); and hence that by the use of plate XIV and the given cross section, it is easy to compare the velocity and corresponding earthy matter held in suspension, with sufficient precision to perceive that the observations of the second year accord with those of the first.

The same remarks apply to the observations of Prof. Riddel; which as they cover the whole period from May 21 to Aug. 13, 1846 (only one week less than three months) appear to us to justify the claim of General Abbot, that his views are supported by three years' observations on the Mississippi. The exact time is 2.9 years.

In respect to the question of the nature of the bottom, the tables in Appendix C, roughly estimated, exhibit about 3,000 casts of the lead between "Cairo and the Gulf" in the Mississippi river; and General Abbot has explained that the entries, showing the nature of the bottom, refer to the bottled samples, and do not include all collected by the lead. Mr. Eads restricts his criticism to the region between "Cape Girardeau and Vicksburg," and quite ignores the latter statement. As General Abbot lays great stress upon the fact that the conclusions announced in the Mississippi Report, as to the nature of the bottom of the river, are largely based upon data other than the soundings, Mr. Eads does not seem to fully understand his position on the question.

As to the matter of the St. Louis bridge, it is fair to assume that Mr. Eads, being the architect, is better informed as to the details than General Abbot; but we must say that the summary of the General's statement, given by Mr. Eads at the close of the letter, seems hardly fair, and we would accordingly refer the reader to the original article in our January number. ED.

THE old spherical weight and drop hook will not be required much longer for breaking castings of large size, as dynamite provides a much more expeditious and economical smasher. Some very heavy old castings belonging to the Hurlet and Campsie Alum Company were recently successfully broken up with dynamite by Mr. Somerville, of the Dynamite Company, Stirling. The castings consisted of large shallow pans from 3 inches to 4 inches thick, and each weighing about four tons. Five cartridges of dynamite were placed in a line from the outer rim of the pan towards the center in three segments, and the explosive was covered up with clay, and fuses applied with detonators attached. On ignition and explosion the pans were most completely broken up.

* These are my italics, but his words.

LINKAGES.

A KINEMATIC DISCUSSION OF THE DIFFERENT FORMS OF ARTICULATED LINKS WITH ESPECIAL REFERENCE TO PEAUCELLIER'S CELL.

By J. D. C. DE ROOS.

Translated from "Revue Universelle des Mines" for Van Nostrand's Magazine.

I.

M. Peaucellier, an officer of engineers of the French army, proposed in 1864, in the *Nouvelles Annales de Mathematiques*, the problem of the transformation from circular to alternating rectilinear motion, by means of articulated rods, and in 1873 he furnished the solution in the same journal.

The arrangement which he devised is extremely simple and has been applied to many problems, both in tracing curves and the mechanical solution of problems. Still it is only quite recently that this beautiful invention has received the attention that it merits, and we are largely indebted to M. Lipkin, a pupil of M. Schebicheff, of the University of St. Petersburg, who published in 1871 in the *Bulletin de St. Petersburg*, a solution of the same problem, entirely independent of the cell of Peaucellier.

Since then the rhombus of Peaucellier was made the subject of a paper by Prof. Sylvester, before the Royal Institution of Great Britain, which was published in many English journals, and was translated for the *Revue des Scientifique* of Nov. 21, 1874.

This paper, very interesting from many points of view, serves as the basis of the present essay.

We shall consider more particularly the direct practical applications of this invention, such as the tracing of conic sections and other curves, rather than its uses in mechanical calculations.

Peaucellier's apparatus consisted essentially of four equal rods or links forming an articulated rhombus, but the name is more particularly applied to the arrangement represented in Figs. 1 and 2, in which the four links are pivoted at the extremities A, B, A′ and C in such manner as to be movable in the plane of the figure, while the two opposite angles A and A′ are also united by links to the axis of rotation O.

The movable joints are indicated in the figures by open circles, and the fixed joints by black ones,

Fig. 1.

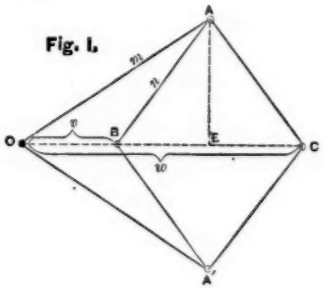
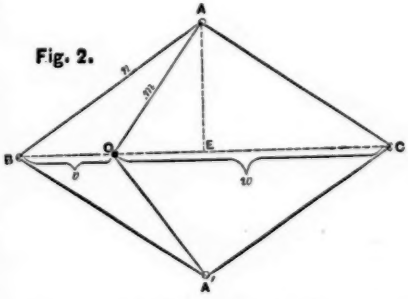


Fig. 2.



The connector links OA and OA′ may be longer or shorter than the sides of the rhombus, as shown by Figs. 1 and 2, and upon this proportion depends the position of O, whether it falls outside or inside.

The system then, is variable at will, so far as the distances OB and OC are concerned, within determinate limits.

Whatever dimensions are assumed for one of these distances, it is evident that the three points O, B and C will fall in a right line, and that the two arms OB and OC depend upon each other.

The remarkable properties of Peaucellier's cell depend upon the simple rela-

tions existing between these arms or links.

From the point A (Figs. 1 and 2) drop the perpendicular AE upon BC, and for the case in which the arms have the same direction as in Fig. 1, OB = + v, and OC = + w; while, for the case represented in Fig. 2, in which the arms have opposite directions, we make OB = - v and OC = + w.

Let m represent the length of OA or OA′, and n the length of each of the sides of the rhombus; we have, when the point O is on the outside:

$$BE=\tfrac{1}{2}BC=\tfrac{1}{2}(w-v)$$

$$m^2=\overline{AE}^2+[v+\tfrac{1}{2}(w-v)]^2$$

and $$n^2=\overline{AE}^2+\tfrac{1}{4}(w-v)^2.$$

When o is within the rhombus

$$BE=\tfrac{1}{2}BC=\tfrac{1}{2}(v+w)$$

$$m^2=\overline{AE}^2+[\tfrac{1}{2}(v+w)-v]^2$$

and $$n^2=\overline{AE}^2+\tfrac{1}{4}(v+w)^2$$

These equations give for the first case;

$$m^2-n^2=vw,$$

and for the second,

$$m^2-n^2=-vw;$$

so that in whatever manner we move the systems, the product of the arms is always constant.

The difference m^2-n^2 is called the *modulus*. Perhaps it would be better to call it *power*, from its analogy to the equation of the hyperbola referred to its asymptotes, and we will represent it by C^2. The power may be positive or negative according as O is outside or inside the rhombus, or whether the arms have the same or opposite directions.

In the first case, the system is said to be *positive*, and in the second, *negative*.

For the greater facility in the calculation, we will henceforth consider the arms as positive, or, in other words, we will multiply the equation $m^2-n^2=-vw$ by - 1, which will give C^2 another signification, and make the difference m^2-n^2, or n^2-m^2 in all cases a positive quantity, whether m is greater or smaller than n.

The difference between the positive and the negative system will be simply that, in one case the arms will have the same, and in the other, opposite directions; and for both cases we shall have the general equation.

$$vw=C^2 \quad (1)$$

As to the limits between which v and w can vary, it is true they are in fact nearly equal, they may, notwithstanding, be extended to any limits between zero and infinity.

Designating by x the least value of v or w, and by the greatest value, we shall have, Fig. 1, $w=x=m-n$ and, Fig. 2, $x=m-n$, while for both $y=m+n$, whence for the positive system

$$m=\tfrac{1}{2}(x+y),\ n=\tfrac{1}{2}(y-x),$$

and for the negative system these values interchanged

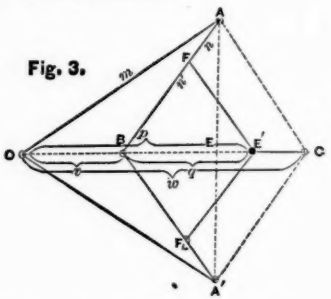
$$n=\tfrac{1}{2}(x+y),\ m=\tfrac{1}{2}(y-x).$$

In taking then m or n at convenience, x and y may take all values between zero, and infinity, and the determination of their limits has no other significance.

Before passing to the applications of these positive and negative systems, we wish to say a few words about a system, by means of which algebraic operations, of which we will treat, can often be simplified.

In Figs. 1 and 2 if we transport the sides AC and A′C, parallel to their original positions, till their intersection falls at some point E′ on the line BC (Fig. 3), then joining, as shown in the figure, the four sides to the points F and F′, there will be formed another rhombus, having for its side BF or BF′ = n', and if we designate by p and q the arms OE′ and BE′, we shall have:

Fig. 3.



because $BE' : BC :: BF : BA$

or $q : w-v :: n' : n$

therefore $q=\frac{n'}{n}(w-v)$

and $p=v+q=v+\frac{n'}{n}(w-v)$

whence $$p+q=v+2\frac{n'}{n}(w-v)$$

$$p-q=v$$

and consequently

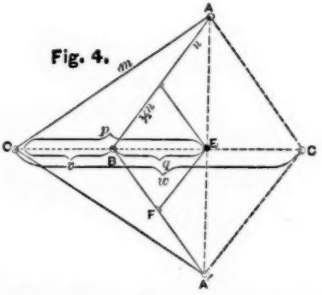
$$p^2-q^2=v^2\left(1-2\frac{n'}{n}\right)+2\frac{n'}{n}vw=v^2\left(1-2\frac{n'}{n}\right)+2\frac{n'}{n}C^2$$

This equation is simplified by supposing, as we shall, in that which follows, that the point E′ coincides with the intersections E of the diagonals of the rhombus (Fig. 4); in other words, taking $n'=\frac{1}{2}n$. In this case the first term of the second member disappears, and there remains

$$p^2-q^2=C^2 \qquad (2)$$

In the transformed system, Fig. 5, it is secondary to change p^2-q^2 to q^2-p^2, unless we wish to assign a negative value to C^2.

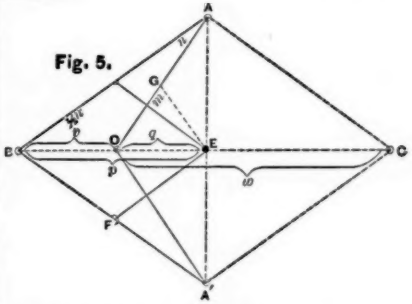
But the direction of p and q in relation to E may become invested when OB increases sufficiently, measuring from O (Fig. 4) or from B (Fig. 5), while the limits of p and q remain the same in both cases.



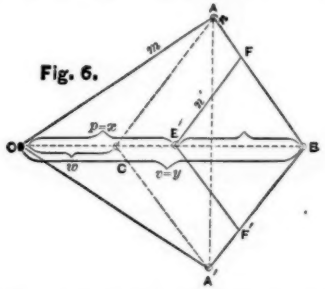
For the applications, however, it is a matter of indifference whether we employ a positive or a negative system, since in the two cases the uniting of the most salient vertices gives the same triangle, or quadrilateral, for the same limits and values of p and q.

There exists only a difference in the dimensions of the cell, and this is merely apparent since the free movement of the basis, not at all embarassed by uniting E to the middle of the connectors.

The limits of p and q are, in virtue of Fig. 4, for the case in which we wish negative values $\sqrt{m^2-n^2}$ and m for p, o and n for q. On the other hand, in virtue of Fig. 5, the inverted values are $\sqrt{n^2-m^2}$ and n for q, o and m for p; furthermore, the values m and n can always be so taken that p and q shall remain within determinate limits.



Besides this system, of which Sylvester first pointed out the remarkable property of making the difference of the squares p^2-q^2 constant, the same author mentions another linkage, by means of which circles of long radius may be traced.



This combination is shown in Fig. 6, but theoretically it does not differ essentially from Fig. 3. In effect, from the moment that OB (Fig. 3) becomes greater than OE′, that is to say, when q becomes negative, Fig. 3 is transformed into Fig. 6, so that the equation

$$p^2-q^2=\left(1-2\frac{n'}{n}\right)v^2+2\frac{n'}{n}C^2,$$

which becomes, after substituting for q $p-v$;

$$pv-\left(1-\frac{n'}{n}\right)v^2=\frac{n'}{n}C^2$$

is equally applicable to the case of Fig. 6.

We will make a special study of this form of linkage, which we will call Sylvester's linkage (losange de Sylvester), since he first gave a description of it in the *Educational Times*. We will designate by x and y respectively the distances $OE'=p$ and $OB=v$ (which have in the case the function of arms), and are measured from O (Fig. 6), so that their general properties are expressed by the equation:

$$xy-\left(1-\frac{n'}{n}\right)y^2=\frac{n'}{n}C^2 \qquad (3)$$

Making C^2 equal to zero, that is to say, making $m=n$, we obtain:

$$y=\frac{n}{m-n'}x$$

and the system is transferred into a pantograph (Fig. 7). Add further to the link E'F' the coupling E'H in the prolongation of F'E', the two bars CA' and A'B can be suppressed, and the system takes the form of the common pantograph (Fig. 8).

Fig. 7.

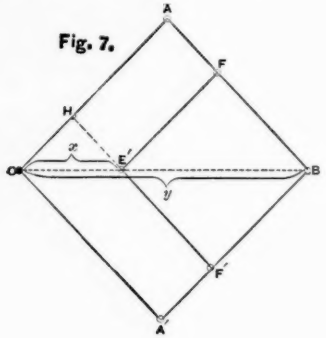
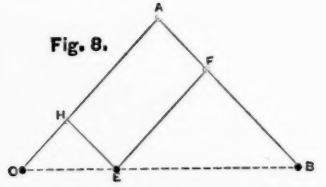


Fig. 8.



Although the systems, which we have already considered, are comprehended in the general arrangement of Fig. 9, of which the links are in pairs of the same length, we have derived from preference one from the other in order to avoid, so far as possible, the change of signs.

Strictly the links should not be in pairs of equal length, but it is evident that when this equality is realized that the three points O, B and C will be found in a straight line. The properties found to belong to Peaucellier's cell are independent of the length of the links. Nevertheless, we will treat from preference of symmetrical combinations.

The systems considered can be coupled together and thus afford new combinations of linkages; and we give the name of *elements* to the primary systems which make up the compound system.

For the applications which we propose to study, it will suffice to employ the Peaucellier Cell. The transformed system (système varé, Fig. 5) and the linkage of Sylvester, the two latter forming, so to speak a single system.

The Peaucellier Cell as an instrument for calculations:

One of the most simple applications of this linkage is that which affords the reciprocal of any given ratio.

Fig. 9.

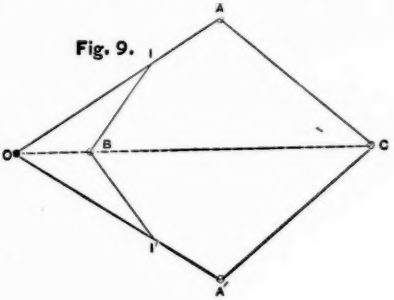
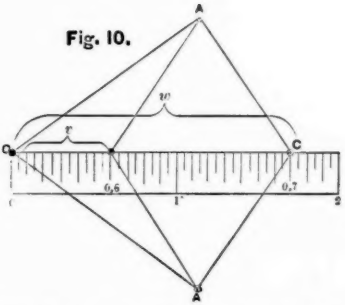


Fig. 10.



If in Fig. 10 a graduated scale OC be placed along the line of the poles BC of a positive losenge, the point C will indicate the inverse value of OB, if the

zero of the scale is placed at O, and if the power C is taken for unity of the scale.

From equation (1) we deduce

$$OB.OC=C^2$$

or $\frac{OC}{C}=\frac{1}{\frac{OB}{C}}$ in which $\frac{OC}{C}$ is the reciprocal of $\frac{OB}{C}$.

When employed thus the system is called a *reciprocator*. The lengths OA and AB (Fig. 10) being measured in millimeters $OA^2-AB^2=625$, and consequently the unit C is the square root of this number, or 25 millimeters. In the negative system the divisions of the scale would have to be continued beyond the zero point. For any other unit c the value of OC would represent $\left(\frac{C}{c}\right)^2$ times the reciprocal of OB.

Although the reciprocator is of but little practical use in finding reciprocals of given numbers, since they are determined so easily by other means, it is capable, nevertheless, of rendering good service in obtaining a series of inverse values, such, for example, as those of consecutive radii vectores of a curve.

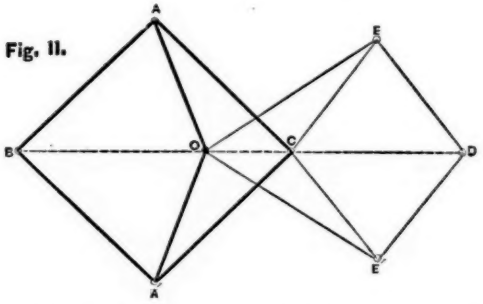
The trace of the inverse of a curve being given, that is to say, of a curve of which the radii vectores have an inverse value, we may obtain them by the pantograph, a trace of the curve on a reduced scale.

Nearly in the same manner, and basing the operation upon the equation $OE=\sqrt{EB^2+C^2}$, we may by means of the transformed system (Fig. 4) extract the square root of a quadratic binomial containining one variable. In this case the point E should coincide with the zero of the scale. The division of the scale, which is regarded as a unit, is a matter of indifference, provided C is measured by the same unit. The length of the connectors should be made variable, and permit giving different values to C. This system may, like the reciprocator, be employed in tracing curves.

Expanding or reducing variable quantities by the Peaucellier Cell is a somewhat more complicated operation, for it requires the joining of two of their elements. In uniting them we superpose the joints O and C as shown in Fig. 11, which comprehends a positive and a negative system. In this compound system the linkages have evidently the same liberty of motion relatively to each other as when the elements are separate; and the reciprocal relations of the distances between the centers of relation of either system, compared with the other, lead to those remarkable properties which result from the combination.

Thus in Fig. 11, having $OD.OC=C^2$ and $BO.OC=C'^2$, whence $OD=\frac{C^2}{C'^2}OB$, we see the arm OD represents a length which is $\frac{C^2}{C'^2}$ times as great as OB, and

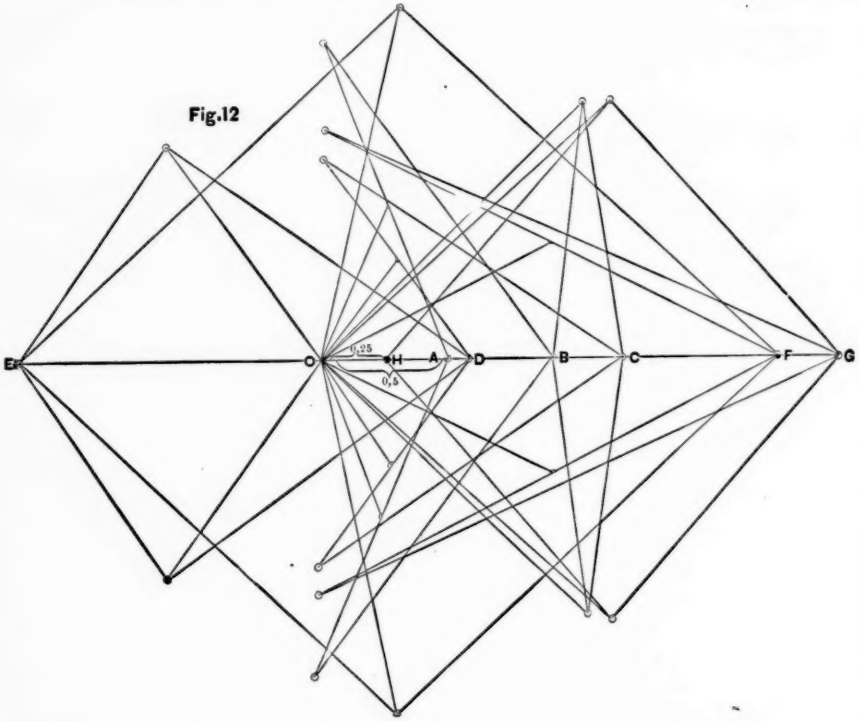
Fig. 11.



this system becomes transformed into an ordinary pantograph by making OC nothing, and letting fall one of the links CE and CA′ upon the prolongation of the other. The same may be said of AC and CE′. It is true that the powers C^2 and C'^2 become nothing, but their ratio is none the less definite and equal to $\frac{FD}{AB}$.

Extraction of square roots, or raising

Fig. 12



to the square, requires a complex combination of linkages which is only of a purely theoretical interest. This combination is made up of four simple Peaucellier elements and three of the *systeme varié* (Fig. 4) joined as shown in Fig. 12. In this combination OA gives the square root of OH, and inversely OH gives the square of OA.

In order to exhibit better the coupling of these elements we have represented separately the upper part of each element on a reduced scale (Fig. 13). The same letters have the same signification in the two figures.

The powers of the different elements are equal among themselves and have the following values, also given in Fig. 13,

$(\frac{1}{2}C\sqrt{2})^2$, C^2, C^2, C^2, $(C\sqrt{2})$, C^2 and $(\frac{1}{2}C\sqrt{2})^2$

It follows from this by reason of equations (1) and (2) and taking $OA=x$ that we have consecutively:

$$OB=\sqrt{x^2+\tfrac{1}{2}C^2},\quad OC=\frac{C^2}{OB}=\frac{C^2}{\sqrt{x^2+\tfrac{1}{2}C^2}}$$

$$OD=\sqrt{OC^2-C^2}=C\sqrt{\frac{C^2-2x^2}{C^2+2x^2}}$$

$$OE=ED-OD=\frac{C^2}{OD}-OD=\frac{C^2}{\sqrt{C^4-4x^4}},$$

$$OF=\frac{2C^2}{OE}=\frac{C\sqrt{C^4-4x^4}}{2x^2},\quad OG=\sqrt{C^2+OF^2}=\frac{C^3}{2x^2}$$

and $OH=\frac{\frac{1}{2}C^2}{OG}=\frac{x^2}{C}$, then $\frac{OH}{C}=\left(\frac{x}{C}\right)^2$

or taking C as the unit of the scale $OH=x^2$; OH represents then the square of OA, and inversely OA is the square root of OH. In Fig. 12 the unit C of the scale being 40 millimeters, $OA=0^m5$ and consequently $OH=0^m25$.

On the other hand, if OB (Fig. 13) represents the cosine of an angle w, it is clear from the preceding that OC, OD,

OF, OG and OH represent the values respectively of

sec w, tan w, tan $2w$, sec $2w$, and $\frac{1}{2}$ cos. $2w$.

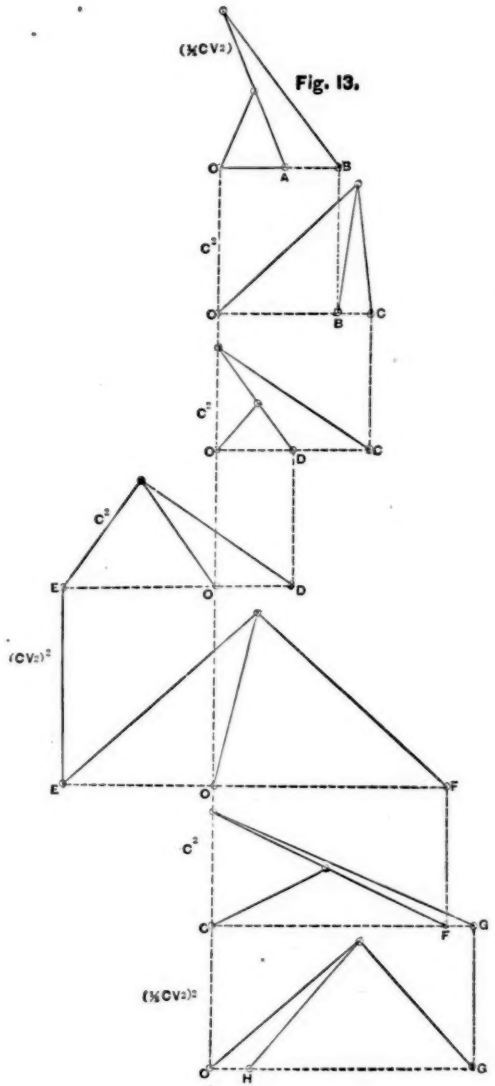


Fig. 13.

THE TESTING OF PIPES AND PIPE-JOINTS IN THE OPEN TRENCHES.

By MALCOLM McCULLCH PATTERSON, Assoc. Inst. C. E.

From Selected Papers of the Institution of Civil Engineers.

The question of water supply is divided into three heads. First, the conservation of existing and known sources; secondiy, the discovery and utilization of new and unknown sources; and, thirdly, the efficient and economical management of the water already under distribution.

Under the third head is classed the present Paper, in which the Author lays no claim to original discovery. He simply wishes to draw the attention of those concerned to certain facts which have come within his own experience, and by discussion thereupon to arrive at some practical result.

Though no Paper upon this subject has, to the Author's knowledge, hitherto appeared before the Institution, the importance and difficulty of securing good pipe conduits has frequently been referred to by hydraulic engineers in discussions upon waterworks, and it is admitted that a large amount of silent and invisible leakage from the mains accompanies most systems of water supply. Among instances of the evil effects of leaky mains may be cited Liverpool, where the consumption of 117,425 persons was reduced from 32.12 gallons to 15.97 gallons per head per day by the repair of leaks, discovered by the waste-water-meter system adopted by Mr. Deacon; Vienna, where a special commission was appointed to report upon the failures in the main line of conduit to that city; and Lewes, in Sussex, where an outbreak of fever has been traced to this cause amongst others.

The experience of most engineers engaged in this branch of the profession confirms these views, the difficulties being the impossibility of obtaining perfect workmanship in the joints and perfect scrutiny of each pipe; and further, if these could be had they would form no safeguard, inasmuch as many hidden defects in pipes are only proved after the caulking of the joints and the application of the water pressure, as demonstrated by the experience gained at Ossett. Examples, therefore, are common, where accidents to conduits, beyond the control of those in charge, cause much damage to the public, and anxiety and unjust blame upon the engineer.

It is proposed to show that, by systematically testing, under proper supervision, the pipes and pipe-joints in the open trenches, a genuine safeguard will be found.

The Ossett waterworks were carried out by the Local Board of the district of Ossett-cum-Gawthorpe, a woollen-manufacturing town on the river Calder, in the West Riding, containing 10,000 inhabitants, and lying upon the Coal Formation, in which no proper supply of water was available. The Author's scheme was accepted in 1874; parliamentary powers were procured in 1875, and the works were carried out in 1876–7, at a cost of £16,707.

The supply is purchased in bulk from the Batley Corporation, and is delivered through meters fixed near the Staincliffe service-tank of that body, at the price of 8*d.* per 1,000 gallons, in a scale of quantities graduated according to the anticipated needs of the population. The water is gathered on the Millstone Grit Formation, near Holmfirth, on the borders of Cheshire, about 15 miles south-west of Ossett, by a system of reservoirs designed by Mr. Bateman, President, Inst. C.E.

The Ossett works consist of a line of pipe-conduit, a covered service-tank, and high and low level distributing pipes.

The conduit is of 10-inch cast-iron socket-pipes, about 3 miles long, having a fall of 56 feet, and is designed to convey 500,000 gallons per day, or 20 gallons per head for the domestic and manufacturing purposes of a population of 25,000. The maximum working pressure is 360 feet, or 150 lbs. per square inch, extra thickness of metal being provided where the head, or water column, exceeds 200 feet. Sluice and air valves are provided, and the whole conduit was

tested at suitable intervals while the trenches were open. Kennedy's meters are used at each extremity of the conduit.

The covered service-tank holds slightly over 1,000,000 gallons, and is distant 2 miles from the center of Ossett. The mean internal dimensions are—108 feet long, by 100 feet wide, by 15 feet deep. The bottom is of cement concrete, rendered with cement mortar; the sides of blocking-faced and rubble masonry retaining puddle walls. The whole area is arched with brick and concrete, turned on cast-iron girders, supported by sixty-three pillars of the same material, and covered with earth. The inlet, outlet (high and low level), and overflow and discharge pipes, are so arranged that the whole district can be supplied without the reservoir, either through the high or low level mains, or each level by either main; and reflux valves are inserted on the inlet pipes, to prevent the emptying of the tank from an accident to the conduit.

The system of distribution is divided into two levels; the high level supplying one-sixth, and the low level supplying five-fifths of the population. The total length of pipes in the contract was 20,289 yards, varying from 9 inches to 3 inches in diameter, the whole being of the ordinary socket kind, with lead joints widening inwards, so as to form a wedge joint incapable of being drawn. Sluice valves are laid at the junction of each street, both on the main and on the branch. Hydrants are put down every 100 yards; air valves where necessary; and discharge or scouring pipes at every depression or dead end. The whole of the pipes were tested as before.

The Author's attention was drawn to the subject of testing the pipes by some remarks made at a Local Government Inquiry by Major Tulloch, R.E.; and a clause was introduced into the specification for pipe-laying, empowering the engineer to test the mains after jointing, in the open trenches—which, with the exception of about two hundred and eighty-eight pipes, was done throughout. The testing apparatus consisted of a common hydraulic-ram pump carried in a wheelbarrow, a few feet of strong hose, and a blank flange, provided with an inlet for and an outlet for air. The blank flange was attached to the socket of the end and uppermost pipe in any length, by a wrought-iron clamp and screw bolts; a pad or cushion of wrapped hemp, with a cast-iron ore, being used for the joint. The water was chiefly obtained from the source of supply. The pressure applied was 50 per cent. above working pressure (not being less than 50 lbs. per square inch), and the length tested varied from 60 to 500 yards, the average duration being about two hours for the small sizes, and five hours for the 9-inch and 10-inch pipes. The inspector, Mr. Marriott, kept an exact record of each test, showing the diameter of pipe, the number of test, the date, the number of pipes in each length, the pressure applied; and finally, what was of the greatest importance, precise details of the character of every failure, either in the casting or in the joint. From this record an analytical table was carefully drawn up, whence is obtained the following summary, showing the percentage of failures of the aggregate of all sizes—

		Number of Failures.	Per-centage.
Total joints tested,	7,763	244	3.14
" straight pipes tested,	7,249	104	1.43
" special " "	514	50	9.72

These results are, so far as the pipes are concerned, solely attributable to concealed defects in the castings, discoverable only after caulking, and while under pressure. Change of weather or other adventitious circumstances did not enter into the cause, although the risks might be lessened by the employment of the best known methods of casting, which it is the object of such systematic testing to ensure.

	£	s.	d.
The cost of the testing engine and repairs was	20	14	8
" " testing the mains "	45	8	10
" " repairs during testing "	195	9	1

The whole of the last item fell upon the contractors who supplied the defective castings. The practical result has proved most satisfactory. The repairs upon the line of conduit since January 1877 have been nil, and those on the distributing mains, since taken in hand by the Board, in January 1878, have cost about £5.

The Author's conclusions are:

1. That the usual mode of laying cast-iron socketed pipes without testing after jointing, and in the open trenches, does not give sufficient security against leakage.

2. That by such testing the desired security may in most cases, if not in all, be obtained at no great inconvenience or cost.

3. That all pipes, special pipes* included, should, if possible, be tested before leaving the foundry.

4. That the value of the principle of casting pipes with the sockets downward is confirmed, although it affords no absolute security against defective sockets.

5. That systematic testing after jointing will tend to improve the quality of both castings and joints, the cost of each failure being thus largely increased, and falling upon the contractor at fault.

6. That the public will be further secured against accidents to pipe conduits, with their attendant evils, viz., the cost of repairs, the loss of water, the interruption of supply, and the indraught of gas or other foul air at leaky points when the pipes are empty.*

REPORTS OF ENGINEERING SOCIETIES.

ENGINEERS' CLUB OF PHILADELPHIA.—*Meeting of June 7, 1879.* Mr J. H. Harden introduced Mr. Joseph S. Paxson, President of the Harrison Patent Car Axle Co. who read a paper describing the axle and exhibiting a working of the same. The main axle is undivided and carries one wheel. From its center to the other wheel it is fitted by a collar which bears on one end of the second wheel. The main axle has two points of bearing in the collar. The axle is thus rigid while each wheel is independent in its action. The extra cost of this axle is fifteen dollars for horse cars, and hundred dollare for eight wheel steam cars. The additional weight in the former instance is about 150 pounds, and in the latter from 500 to 600 pounds for each car. Tests were made on a street railroad in New York City, which showed that on a curve of 40 feet radius, 57.42 per cent. more can be pulled by a given force when the Harrison axle is placed on the car than where the ordinary axle is used. An improved nutlock used in securing the car-couplings in this axle was shown and described. Mr. Gorman, introduced by Mr. Hering, exhibited a trap designed to prevent the egress of noxious gases from sewer inlets. It consists of a valve which falls from its seat as soon as the weight of the water upon it exceeds that of its counterbalance. It is claimed that when the valve is open the water still forms a tight joint between it and its seat, as the water flows out, the valve comes back to its seat gradually, always preserving the water joint. Mr. Thomas M. Cleeman read a paper showing that from experiments made by Mr. Darrach, Kutter's formula was not applicable to cases in which the flow of water was under pressure. The paper was discussed at length by Messrs. Hering and Cleeman. The Club adjousned until October next.

* In the original Paper are given the record of tests, the analysis thereof, and a schedule showing the details of the pipes.

IRON AND STEEL NOTES.

THE FUTURE OF THE IRON TRADE.—The Earl Granville is not an iron and coal-master who believes that England's decadence in the iron and coal world has begun. Addressing other ironmasters at a banquet in Wolverhampton last Friday, he reminded them that regularly since 1872 the reduction in the make of iron the world through had been 800,000 tons per annum. Yet of this reduction only 160,000 tons had fallen to this country, whose diminished ontput had therefore been no more than one-fifth of the whole. If, considering how severe had been the depression, this was not encouragement enough in anticipation of the future, then his lordship would have them bear in mind what had happened since the middle of this century. At that earlier date England was producing just one-half of all the iron made the whole world over. That in the interval millions have been spent in the manufature of iron by the different nations of Europe, and by America, is notorious. Lord Granville deems it most satisfactory, that under such circumstances the proportion of the whole iron consumption of the world supplied by England is at the present day 48½ per cent. We are, therefore, now making less proportionately than at the earlier date by merely 1½ per cent. Since, upon coming of age his father gave him an interest in ironmaking and coal mining, Lord Granville has seen more occasions "than he can count upon his fingers when prices were very low, and there were especial reasons why they could never be high again; and where, on the other hand, prices being very low, it was mathematically demonstrated to him that they could never fall as low as they had been before!" Notwithstanding the steel question, his lordship has, therefore, no fear of history repeating itself; nor has he any fear of the competition of countries who are weighted with the cost of heavy standing armies or prohibitive tariffs; and he looks upon the improved trade in the United States as indicative of the early revival of commercial industry in this country. Lord Granville's views are as valuable as they are interesting, and they are supported in some part by the nature of the Board of Trade returns which have been issued this week.—*Engineer.*

STATISTICS OF THE AMERICAN IRON TRADE IN 1878.—*Abstract of the Report of the Secretary of American Iron and Steel Association, James M. Swank.*

The appended table shows in tons of 2,000 pounds the production of all kinds of iron and steel in the United States from 1872 to 1878. In nearly all of the branches of the domestic iron and steel industries which are here enumerated there has been an increased production

in 1878 over 1877; but, as will appear farther on, this increase in production has been accompanied by a decrease in prices. At no time in the history of the country have prices for iron and steel been so low as they were in 1878, excepting in colonial days, when the price of pig iron was still lower.

PRODUCTS.	1872	1873	1874	1875	1876	1877	1878
Pig iron	2,854,558	2,868,278	2,689,413	2,266,581	2,093,236	2,314,585	2,577,361
All rolled iron, including nails and excluding rails	941,992	1,076,368	1,111,147	1,097,867	1,042,101	1,144,219	1,232,686
Bessemer steel rails	94,070	129,015	144,944	290,863	412,461	432,169	550,308
Rails of all kinds	1,000,000	890,077	729,413	792,512	879,029	764,709	882,685
Crucible cast steel	29,260	34,786	36,398	39,401	39,382	40,430	42,906
Open-hearth steel	3,000	3,500	7,000	9,059	21,490	25,031	36,126
All other steel, except Bessemer	7,740	13,714	6,353	12,607	10,306	11,924	8,556
Bessemer steel ingots	120,108	170,633	191,933	357,517	525,996	560,687	739,226

The Production of Pig Iron in 1878.

The year 1878 witnessed an increased production of pig iron in the United States over the year 1877, as 1877 had witnessed an increased production over 1876, which was the year of lowest production since the panic of 1873. The production of pig iron in 1878 was 2,577,361 tons of 2,000 pounds, and from 1872 to 1878 it has been as follows.

1872	1873	1874	1875
2,854,558	2,868,278	2,689,413	2,266,581
1876	**1877**	**1878**	
2,093,236	2,314,585	2,577,361	

The increase in 1877 over 1876 was 221,349 tons; and in 1878 over 1877 it was 262,776 tons. If a similar rate of increase be maintained in 1879, as we have no doubt it will be, the production in this year will equal that of either of the exceptionally productive years, 1872 and 1873; while a much less rate of increase will carry our production in 1880 above 3,000,000 tons. Joined to the reduction in stocks of pig iron which took place in 1877 and 1878, and which will be shown farther on in this report, the fact is fully established that the consumption of pig iron in this country has greatly increased since 1876. This increased consumption is not wholly accounted for by the growth in the past two years of the Bessemer branch of our steel industry, rapidly as it has been developed; and it is to be noted also that the production of iron rails and all forms of rolled iron was less in 1877 than in 1876, and but little greater in 1878 than in 1876. The inference is therefore clear that, although the increased consumption of pig iron in the last two years was due partly to the activity in our steel works, it was also partly due to an improvement in the business of the foundries and machine shops of the country, and but slightly due to the iron rail mills and other iron rolling mills.

If the prices obtained for pig iron in the last two years had kept pace with the increase in production, the pig iron branch of the American iron industry would have been in a reasonable degree prosperous; but unfortunately we are not permitted to record a result which would have been so gratifying. Prices of pig iron declined steadily in 1877 and 1878, as they had previously declined since 1872, in which year the highest prices were attained since 1865, the last year of the war. The average yearly price of No. 1 anthracite foundry pig iron at Philadelphia from 1872 to 1878 was as follows per ton of 2,240 pounds.

1872	1873	1874	1875	1876	1877	1878
$48 87½	$42 75	$30 25	$25 50	$22 25	$18 87½	$17 62½

From 1872 to 1876 the price declined almost 55 per cent., and the total decline from 1872 to 1878 was 64 per cent. The highest price quoted in 1872 was $53.87½, and the lowest price quoted in 1878 was $16 50 in November. Since that month the price has fluctuated, but has at no time fallen below the figures which were then quoted, and since February it has slightly advanced.

The use of coke as a blast furnace fuel has steadily increased during the past few years, but the most significant feature of this increase is the invasion by coke of districts heretofore exclusively appropriated by anthracite coal. Connellsville coke is now used in some furnaces in New York, New Jersey, Eastern Pennsylvania and Maryland, but in every instance as a mixture with anthracite. The furnaces which use this mixture we have classed as anthracite furnaces. If an absolutely accurate classification of furnace fuels were possible it would probably appear that more pig iron is now made with bituminous coal and coke than is made with both anthracite and charcoal combined.

The States which increased their production of pig iron in 1878 were Vermont, Connecticut, New York, New Jersey, Pennsylvania, Virginia, Georgia, Alabama, West Virginia, Kentucky, Tennessee, Ohio, Illinois, Wisconsin and Oregon. Those which decreased their production were Maine, Massachusetts, Maryland, Michigan and Missouri. North Carolina, Texas

and Indiana made pig iron in 1877, but made none in 1878. None of the Territories made any pig iron in 1877 or 1878.

Production of Iron and Steel Rails in 1878.

The production of rails of all kinds in the United States in 1878 was 882,685 net tons. This product has been but twice exceeded in the history of the country, in 1872 and 1873.

The increased production in 1878 over 1877 was 117,976 tons. The product of 1878 was composed of 550,398 tons of Bessemer steel rails; 322,890 tons of iron rails; and 9,397 tons of open-hearth steel rails. The production of Bessemer steel rails was 118,229 tons greater in 1878 than in 1877; while that of iron rails was 9,650 tons less. Open-hearth steel rails appear in the statistics of last year's rail production for the first time. In 1879 we will probably produce as many rails as in 1872, when we made 1,000,000 tons.

RAILWAY NOTES.

THE North Metropolitan Tramways Company has determined upon an extension of their lines in the east of London, with a view of rendering the network complete. The first new line is from Grove road, through Victoria Park, and the other is in Grfiham road, Dalston.

AT a meeting of the Victorian Amalgamated Society of Engineers, Mr. Woods, Minister for Railways, said that since he had assumed office he had done all in his power to have work performed in the colony, instead of sending home for it, and £724,000 has been thus expended.

FOR some time it was suspected by the officers of the Frankfurt-Bebra Railway that the practice of casting white metal bearings round steel axles had been the primary cause of several breakages, and the experiments were undertaken to ascertain the possibility of such being the case. From vol. lvi. of the "Proceedings" of the Institution of Civil Engineers, p 321, we lean that by repeating the casting process a number of times on the same axle, it was found that in every case, and with every kind of steel tried, cracks appeared sooner or later, sometimes on the second trial, sometimes not till the forty-fifth. The white metal used contained 7 parts copper, 14 parts antimony, and 79 parts zinc, and melted between 450 deg. and 500 deg. C., 810 deg. to 932 deg. Fah. These experiments tend to show that the common plan of casting white metal round the axle is a fruitful source of danger.

THE modern has been brought into contrast with the old in Rome by the opening on Sunday last of a steam tramway to Tivoli, connecting the capital with the oil-producing country. The line follows the old high road well known to British tourists, and presents the same ugly curves, which will be rectified. Leaving on the left the sulphur baths described by Lyell, where an establishment will shortly be erected, and on the right Hadrian's villa, the line passes on to Tivoli. The ceremony took place under the auspices of the Minister of Public Works; M. Tant, the director of the company; the Perfect and Syndic of Rome, and several senators and deputies. Two engines, drawing three cars seating forty persons, left the station at 1 p. m., and arrived at Tivoli at 3 p.m., having passed over the ground easily, and taken the steepest ascents in perfect safety, at a rate of 15 kilometers an hour.

FROM a number of experiments on railway axle friction made some years ago, it was concluded that (1) the friction increases with the load, and in a considerably higher ratio. (2) That the friction increases with the velocity, but not in the same proportion as with the load. (3) That gun-metal bearings have been found to produce the least friction, but that they require more careful fitting. Various iron and cast steel axles were employed for the experiments, all having been in use for some time. The journals of the iron axles were 3.15 in. diameter by 5.6 in. length. Those of the cast steel axles were 2.8 in. by 5.6 in. The speed was 180 and 360 revolutions per minute. The loads were 321, 631, 941 and 1251 lbs. per journal. One gun-metal, one tin and one lead alloy were used for the brasses. The inferences drawn from those experiments were as follows:—(1) The coefficient of friction for iron and cast steel axles, when lubricated with rape oil or colza oil, and with bearing of tin alloys or hard lead, is between 0.009 and 0.0099. (2) For the same axles, with gun-metal bearings, the coefficient is 0.0141. (3) The coefficient of friction, for such loads as occur on railway vehicles, is independent of the load, consequently a smaller or larger bearing surface of the journals is (within certain limits) of no influence on the friction. (4) The coefficient of friction, for such velocities as occur in railway vehicles, is independent of the velocity. (5) When grease is used as the lubricant; the coefficient of friction is greater than with oil for small loads, but for heavier loads, where the journals become warm more quickly, the coefficient of friction is the same for grease and oil, except at starting Different sets of experiments have given different results. By some experiments made on the London and South Western Railway, to determine the resistance with oil as compared with grease, axle-boxes and carriages weighing about 6 tons 4 cwt., the tractive force, in pounds per ton weight, required to keep them in motion was as follows, for seven different experiments: 2.8, 2.6, 2 9, 2.2, 4.0, 2.6; giving a mean of 2.85 (Proc. I.C.E., vol xxviii). The bearings were of white metal and the loads small, both of which circumstances would tend to diminish the resistance. This average gives a coefficient of 0.0127 for oil axle boxes with white metal bearings. More recent experiments have given, it is said, still lower results for oil, so that on the whole we may safely assume that the coefficient of friction for gun-metal bearings well lubricated with oil is 0.014, and for white metal bearing 0.012.

ENGINEERING STRUCTURES.

THE ENGLISH TUNNEL.—We give some details of the explorations which have been made for the proposed tunnel under the English Channel. M. Larousse, the eminent hydrographic engineer, whose name is already well known in connection with the works of the Suez Canal, has been charged with the careful survey of the bed of the channel, with the object of determining the configuration and the geological character of the rocks of which it is composed. Numerous soundings have already been made, not only in French waters, but also in the parts of the Straits near the English coast. 1,525 observations have been made in this way of the Channel bed district, on the French side of the Channel, and extending to a distance of 17 miles from the coast; from these 753 samples of the sub-marine strata have been collected and carefully arranged, by means of which the boundary of the calcarious and the argillaceous beds have been determined. These sounding operations, which have been many times repeated, have touched the bottom of the Channel no less than 7,671 times, at an average distance one from another of from 100 to 200 meters, and have furnished 3,267 geological specimens. The sub-marine region has been explored in lines nearly parallel to the coasts, and at a distance of from 250 to 300 meters. The width of the Channel between the extreme ends of the proposed tunnel, viz., between Sangate, near Calais, and St. Margaret's Bay, a little to the east of Dover, is about 25 miles. The result of the soundings alone would not have proved sufficient without taking account of the influence of the variations in the level of the water. This important element in the calculations has been studied exhaustively by M. Larongse, whose conclusions are favorable to the project set on foot by M. Lavalley, the celebrated engineer of the Suez Canal, the promoter of the idea of a port of La Reunion. The tunnel will have a length of 36 kilometers and, with a traverse, a continuous bed of gray chalk. While allowing the necessary incline, to insure the running off of the small quantity of water which filter into the tunnel, its depth below the sea-bed will in no part of its course exceed 70 meters. A railway train leaving Paris would enter the tunnel at Sangate, proceed under the Straits and ascend the incline on the English side insensibly, emerging from the tunnel at St. Margaret's Bay, about 6 kilometers from Dover.

THE DARIEN SHIP CANAL.—This project has entered another stage, and an important one. A congress met in Paris on Friday last week, under the presidency of M. de Lesseps, for the purpose, if possible, of finally settling the question. M. Lesseps' name is a tower of strength for such an undertaking, and he is supported by many distinguished representatives of science and commerce from every country in Europe, from America, and even from China. The congress has before it seven distinct plans, including a level canal without locks or tunnel, and one which involves the cutting of a tunnel ten miles long, and high enough and wide enough to allow of the passage of a three-master. A broad and deep trench, like the Suez Canal, is impossible; from the contour of the country, which has been thoroughly surveyed, open cuttings would involve an enormous cost; while a complicated system of locks would interfere with navigation so much as to limit the number of ships passing below the number that is required to make the speculation a paying one. So far back as 1866 it was estimated that the commerce of England, the United States and France, that would be attracted to this route would be upwards of three million tons, some placed it as high as seven; and the lowest figure for the first year's revenue was put at a million and a half; but the work would probably cost two hundred and fifty millions, and the annual outlay would be no trifling sum. At the second meeting of the congress it was shown to be necessary for the permanence of the undertaking that six million tons of shipping should pass through yearly, and that a single lock would reduce the capacity of the canal by one-half. The undertaking, even on the cheapest scale, would tax the resources of the civilized world to complete; but the present congress may be expected either to show its practicability or to prove that it is impracticable. Of the great importance of such a passage to the commerce of the world there cannot be two opinions; it would shorten the passage from the Channel to the chief ports on the western coast of America by from 5,000 to 3,000 leagues. The French Convention once proposed to starve out the English aristocracy, by cutting through the isthmus and sending the Gulf Stream into the Pacific; the proposed canal, if carried out, will modify the course of traffic to America and the East almost as much as these wiseacres hoped to modify our English climate.

ORDNANCE AND NAVAL.

MEASURING THE RECOIL OF GUNS AND THE MOTION OF PROJECTILES.—To know precisely the law of motion of recoil of guns in the first instants after inflammation of the powder is very important for artillerymen, as the pressures in the bore may be thence deduced, and guidance is had in calculating the forms and thicknesses of guns and in choice of powder giving the best effects. Several apparatuses have been devised for the purpose, but lacked the desirable precision. This seems now to have been gained, however, by M. Sebert, in the so-called *velocimeter*, which he has recently brought before the French Society for Encouragement of National Industry. The exact motion of the gun in its recoil can be given in intervals as small as $1_{\frac{1}{100}}$th of a second, or less. The apparatus consists of a strip of flexible steel, movable in a horizontal slide on a board supported in a fixed position. This strip is connected by means of a flexible, but inextensible steel wire with the gun or its carriage, the movement of which it follows. Its upper surface is blackened with soot, and over it is fixed a tuning fork, kept electrically in vibration by a method of M. Deprez. One of the two branches of the fork (which are parallel hori-

zontally) carries a small steel style, and the fork can be turned on a horizontal axis, so as to bring this style into light contact with the black surface. As the strip is drawn along by the gun in its recoil, a wavy line is formed on it by the style, and by comparing the lengths of the successive waves, the travel of the gun in time intervals corresponding to the known times of vibration of the fork can be precisely ascertained. The weights of the masses moved being known, the force applied to the system, and so the pressure in the bore can be determined. The apparatus clearly proved a fact known before—that the velocity of recoil continues increasing considerably after the projectile has left the gun (reaching a maximum in one case when the shot was 15 meters from the mouth). This is evidently due to the expansive force of gases remaining in the gun after the projectile has left. M. Sebert adds to the apparatus what renders it a chronograph for determining the time of passage of a projectile either in the bore or in the air. A number of small electric registers are placed near the tuning fork: they are formed of electro-magnets (of special character), of which the armature, acted on by an antagonistic spring, carries a small steel style producing a continuous trace on the blackened strip, which trace, however, is suddenly displaced when the armature is actuated by rupture of the circuit of the electro-magnet. Such rupture is effected by the projectile in its passage. A special interrupting arrangement is fixed in the mouth of the gun, and other interruptions occur when the projectile passes through frames placed in its course. Each of these effects is registered on the strip. These registers (M. Deprez's invention) are subjected to a very slight retardation, which had to be remedied, considering the precision required of the apparatus, and M. Sebert succeeded in doing so in a way we need not here stop to describe.—*Iron.*

STEEL VESSELS FOR THE BRITISH NAVY.—Practical experience having shown steel, under the modern processes of manufacture, to be adapted to a variety of purposes for which it was at no very remote period considered to be altogether unsuited, it is not surprising to learn that it is superseding iron in multifarious directions, and is being largely used by the Admiralty in the construction of vessels for the British Navy. Indeed, that Navy already possesses a steel despatch vessel, the Iris, which is only waiting to be commissioned, and is regarded by some of the best authorities not only as a strikingly pretty, but also as a remarkably useful craft; a sister ship, the Mercury, is rapidly approaching completion; and six steel and iron corvettes, costing about £100,000 each, and built on the Clyde by Messrs. Elder & Co., three being engined by that firm, and the remainder by Messrs. Humphrys, Tennant & Co., have also been received at the Government dockyards, and are now being masted, rigged, and finished for sea. More recently still, designs have been prepared for the construction of two steel vessels—the Canada and the Cordelia—which are each to be advanced 177 tons this fiscal year, their frames being already laid n the building slips at Portsmouth Dockyard. All the names of these eight corvettes commence with the third letter in the alphabet, those launched from the yard of Messrs. Elder having been named the Comus, Champion, Carysfort, Curacoa, Conquest, and Cleopatra. These six resemble each other in all respects, their principal dimensions being—length between perpendiculars, 225 ft.; extreme breadth, 44 ft. 6 in.; depth in hold, 21 ft. 6 in.; and tonnage 1270. The load draught of water will be, forward, 17 ft., and at after end, 18 ft., 6 in. The engines are compound, and have an indicated horse power of 2,000, while the vessels have six boilers each, and the propelling power is a single screw with two blades, of a diameter of 16 ft. 6 in. The diameters of the cylinders are, high-pressure, 36 in.; low-pressure, 64 in.; and length of stroke, 2 ft. 6 in. The coal-carrying power is 360 tons, and at the trial of the Curacoa the consumption per hour per horse power at full speed was about 8 lbs., and the mean number of revolutions of propeller 105. The vessels are entirely of steel, with the exception of the beams and frames, which are of iron; of the decks and minor fittings, which are of wood; and of the prow, stem, stern-post, and after-part of the keel, which are solid metal castings of great weight, the prow and stern being each cast in one piece weighing about 10 tons. There is a steel deck, 1½ in. in thickness, over the engines, boilers and magazines, and at about 3 ft. below the load water-line. Five water-tight bulkheads extend from the keel to the upper deck, and cofferdams of iron bulkheads, 2 ft. asunder, are built round the hatchways, or openings to the engines, boilers, and magazines, for the purpose of keeping out shot which had passed through the deck or side of the vessel. The hull plating is of steel, and $\frac{5}{8}$ in. in thickness, and is coated with two thicknesses of wood, 3 in. and 2 in. respectively, making a total wood covering of 5 in. Over this copper sheets are nailed, and it may be added that the fixed metal prow of each vessel, being intended for ramming purposes, is made extraordinarily strong with bulkheads, girders, and the like. To all intents and purposes the Canada and the Cordelia will be companion vessels to the six corvettes launched from the yard of Messrs. Elder. A comparison of the specifications shows them to be identical, so far as the principal dimensions are concerned, and in the minute details no fundamental deviations would appear to be intended. At the same time, some particulars respecting these two vessels of the future cannot but possess interest. The steel vertical keel is to be formed of intercostal plates $\frac{3}{8}$ in. thick, single-riveted to a continuous longitudinal plate 22½ in. deep by $\frac{3}{8}$ in. thick. The intercostal portion of the keel is to have a single angle bar of steel on the outside riveted to it, and to the flat keel plate with $\frac{3}{4}$ in. rivets, this angle bar to have its horizontal flange worked continuously, but its vertical flange intercostally between the frames. The continuous longitudinal plate is to have continuous double angle bars of steel on its upper edge. The wood keel is to be of English elm, sided 14 in., to be well secured to the flat keel plate with copper bolts, clinched on metal rings. The false keel is also to be of the same wood, 4 in.

in thickness, and the stem is to be formed of a strong brass casting, to extend from the forecastle to the forefoot, where it is to scarpe with the wood keel, and likewise to make a good connection with the flat keel plate. It is to be rabbeted to receive the ends of the bottom plates and the forehoods of the wood sheathing, at the head to be 7½ in. thick, and at the forefoot of such a thickness as will make good work with the wood keel, and to be of sufficient molding to house the ends of the bottom plates and wood sheathing. The rudder is to be of English oak or teak, sided at the head 15 in., at the upper pintle 14 in., at the keel 11½ in., and at the center of the screw of the size required to receive the feathering shaft. The after body-post, with its boss and flanges, the fore body-post, with its flanges, and the after piece of keel, are each to be cast in one piece, the fore body-post to be sided 20 in. at the keel. The rudder is to be so fitted that it may be shipped or unshipped afloat, and a spare rudder is to be fitted under the propeller shaft. The first and second longitudinals are to be made of intercostal plates, single riveted to a continuous longitudinal plate, like the steel keel; the third longitudinal by the bunker bulkheads, and the fourth are to be of intercostal plates, extending 2½ in. within frames, so as to be riveted to the continuous longitudinal angle bars. The continuous plates and angle bars of the longitudinals are to be wrought to the greatest length procurable, and the first and second longitudinals are to carry their full depth as far through the length of the engine-room, magazines, and boiler-rooms as is practicable, and to be diminished at the extremities. The transverse frames are to stand square to the load water-line and to be 3 ft. 6 in. apart from center to center throughout the ship. The outside plating is to be of steel, worked, excepting on the top-side plating, lap-jointed throughout, and having solid liners where required. The top-side plating is to be flush jointed, and the plates are to be in lengths of 14 ft. and upwards. The butts of the plates are to be as nearly as possible in the middle of openings of the frames, and there are to be two plates between the butts, placed vertically over each other. The whole of the surface bottom is to be sheathed with wood, worked in two thicknesses, the inner one to be 3 in. thick, and the outer one 2½ in., both thicknesses to be worked fore-and-aft, breaking joint, the inner thickness to be fastened with iron screw bolts, spaced and carefully secured through the skin plating, the bolts being made thoroughly watertight. The top-side is to be covered with steel plates $\frac{1}{16}$ in. thick, worked vertically, the plates to be 3 ft. 9 in. wide, and each to be stiffened by a single steel angle bar worked vertically on the frame sides of the plates; the top sides to be strengthened in the wake of the boat's davits, cat-heads, chain plates, &c. There are to be transverse watertight bulkheads formed of steel plates and angle bars to be bounded by the outside plating of the bottom, and of the upper and lower decks. There will be a shaft passage and a passage between the engines and the boilers, and they are to be of steel, with plates ¼ in. thick, except the lowest strakes at the fore-end of the shaft passage, and the lowest strakes of the passage between the engines and the boilers. The engine, boiler and shaft bearers are to be of steel, and the beams of the poop and forecastle, the upper deck, part of the lower deck, and the platforms in the hold are to be of solid steel, welded or rolled. Brown & Harfield's wrought iron capstans are to be supplied to the vessels, which are also to po-sess five Downton pumps, while a steel-rifle-proof conning tower is to be fitted on either side of each ship, and due provision is to be made for drainage, pumping, and ventilation arrangements. The Cordelia and the Canada are estimated to cost rather over £160,000 each, but the actual expenditure is more likely to be in excess of £200,000. The rapidly increasing use of steel in the British Navy, supplemented by its extended application to general purposes, promise an auspicious future for manufacturers of this metal; but it is yet more than hypothetical whether steel will ever entirely supersede iron, which is on all hands acknowledged to possess certain peculiarities which render it specially suitable for certain kinds of work.

BOOK NOTICES

ELEMENTS OF THE MATHEMATICAL THEORY OF FLUID MOTION. WAVE AND VORTEX MOTION. By THOMAS CRAIG, PH. D., Fellow in Physics in the Johns Hopkins University. Science Series, No. 43. New York: D. Van Nostrand. Price 50 cents.

This is a reprint of the series of articles lately published in this Magazine, and which have widely attracted the attention of mathematical students. It is quite unusual to find so heavy a treatise in so small a volume, but it is hoped that engineering students, who delight in analytical investigations, may be induced to regard with favor this contribution to the literature of so difficult a subject, and thereby aid in advancing this department of scientific writing.

The author's preface thus explains the relation of this little book to former treatises, and to a more extended one proposed for the future:

"The subject of Hydrodynamics embraces many of the most difficult problems in the range of physical research.

"Although, at all times attracting the attention of the greatest minds, it is only within little over a century past that much real progress has been made in the solution of the many and complicated cases presented by the ordinary phenomena of Fluid Motion. The names of Euler, Lagrange and Laplace in the last century, and of Helmholtz, Stokes, Thomson, Rayleigh and Kirchhoff in this, stand out preeminently as those that have done the most to advance the theory to its present position. The object of the following article is to present in a short space the more important points in the Mathematical Theory of Fluid Motion, as it has been developed by these investigators. It is a want severely felt by any one making a study of this subject, that there exists no separate and complete treatise on Hydrodynamics.

"It is a fact, I think, greatly to be regretted,

that the men who do the most for the real advancement of science so seldom present to the world the result of their labors and extensive knowledge, in any other form than an occasional memoir in a scientific journal, or in a communication to a learned society. There are, however, notable exceptions to this general rule, as witness: Maxwell's treatise on Electricity and Magnetism; Rayleigh on Sound, Cayley's Elliptic Functions, and a few others. If some one would present to the public a treatise on Hydrodynamics, of the scope of those mentioned on other subjects, he would certainly receive the gratitude of all physical students, and confer a great boon upon the scientific world."

THERMODYNAMICS. By HENRY T. EDDY, C. E.; Ph. D., University of Cincinnati. Science Series. New York: D. Van Nostrand. Price 50 cents.

The science of Thermodynamics is found to have so many applications that of late there has been quite a brisk demand for a compact and clear statement of its laws, and more particularly for the true signification of the laws as rendered by previous writers.

Compressed Air Motors, Ice Machines, Pneumatic Drills and Hot Air Engines, have failed everywhere of accomplishing even a fair degree of success, simply through a want of knowledge of this science, or rather, perhaps, through ignorance of the fact that thermodynamic laws applied to these machines at all.

Prof. Eddy brings unusual skill to bear upon the labor of expounding the principles which, although treated by Rankine and Zeuner, have been found to be too difficult for the average student.

AMES' ALPHABETS. New York: Bicknell & Comstock. Price $1.50.

In addition to an unusual number of letter designs, there is a collection of map borders and topographical signs for the use of map draughtsmen, also monograms for the use of designers.

The specimen alphabets are beautifully printed on tinted paper, and include styles of every variety of elaborate finish, from the plain "marking" letter to be made with the broad-pointed pen to the most ornate capitals employed by the designer or draughtsman.

A DESCRIPTIVE TREATISE ON MATHEMATICAL DRAWING INSTRUMENTS, WITH HINTS UPON DRAWING AND COLORING. By W. FORD STANLEY. Fifth Edition. London: E. & F. N. Spon and W. F. Stanley. 1879. Price $2.00.

The fact that this is the fifth edition of this book is, perhaps, sufficient indication of its value. It is a very carefully written description of all the instruments necessary for making every kind of mechanical and architectural drawing, and though many of these instruments are of a simple character, there are some of a complex nature, the use of which could not well be learned without a clear exposition, such as is here given. The author does not, however, confine himself to a description of the instrument, but gives much useful information on the construction and qualities of instruments, and hints for guidance in making a judicious section of instruments to suit special requirements. Points which would not be noticed by a draughtsman or student are referred to, so that he learns what to avoid and what to look for. The remarks on drawing and coloring have secured the book much patronage for preceding editions; and the author has now added to these some further hints which have increased their value. The high character of the instruments made by the author, with many recent improvements, is sufficiently known and appreciated to make it unnecessary to give here any description of them, but it may be remarked that the present edition of his book describes a number of new instruments of ingenious and simple design for producing similar and opposite curved forms, for copying drawings, and for measuring areas. The improvements shown in compasses illustrate the great advances which have been made during the past few years in these, the most used of all the instruments. With these there is not any excuse for making badly executed drawings. The arrangements adopted for keeping the lower limbs parallel at different distances of separation are simple and effective, and this alone obviates the large center holes which could not be avoided with the old instruments except by the use of center plates. There are some students who only need to have the best instruments placed in their hand to be able to use them to effect, but these are few, so that it may be said that to all students learning mechanical or architectural drawing and coloring Mr. Stanley's book forms a necessary guide.

THE TELEPHONE, THE MICROPHONE, AND THE PHONOGRAPH. By COUNT TH. DU MONCEL. C. Kegan Paul & Co., London. 1879. Price $2.00.

So great has been the interest excited by the discoveries of the telephone and microphone, not only in the ranks of those whose lives are wholly devoted to scientific pursuits, but in every class of the community that reads at all, creating a taste for more knowledge on the subject, that a book of this kind should be acceptable to many. Count du Moncel is known as one of the most copious and at the same time one of the most painstaking of writers on electrical subjects. His works have become classic in France, and are too little known in England. His thorough knowledge of electrical subjects enables him to present his information in a very attractive form; and it may be taken for granted that his descriptive writings are correct descriptions of the instruments about which he is writing. There are differences of opinion as to questions of theory even among the most eminent electricians, and this must necessarily be so till the whole subject is understood.

The work before us is an authorized translation from the French, but it can hardly be said to be an exact translation of the original, because many paragraphs have been added and some have been left out, in accordance with more recent information that was obtainable when the original was written. Commencing with references to the earliest known remarks made as to the transmission of sound to a distance, the author rapidly carries us on to the time when such ideas were experimentally reduced to facts—to the time of Reiss, of Elisha

Gray, of Bell, Hughes, and Edison. The work of these, and of a score of others, is explained, and the position of each in the long roll of experimentalists truly indicated.

MISCELLANEOUS.

A TRAMWAY FOR ADRIANOPLE.—A letter from Adrianople says that a project is on foot for the construction of a tramway from the Karagatch Railway Station to the town. The road is level, and the only difficulty is the Maritza, the existing stone bridge over which is too narrow to allow room for a tramway line on its present platform. It is proposed, therefore, to turn to account the massive stone abutments which project on the upper side of the bridge, and to build a platform for the tramway upon them.

A PARTY of gentlemen went recently to North Woolwich, for the purpose of making a trial trip on board the Castalia, with Mr. Aston's patent narrow-bladed paddles. The vessel, it will be remembered, plied between Dover and Calais, and damaged herself by dashing into the Calais Pier. The bottom of the Castalia was very foul, as she has been lying off Woolwich about seventeen months. The machinery had not been worked all that time. However, notwithstanding all these disadvantages, the paddles gave good results, for although the steam produced was not capable of giving more than twenty-four revolutions per minute, the paddles propelled the vessel upwards of nine knots an hour. This, under a more favorable state of things, could no doubt be increased.

SEVERAL entries had been made for the trial of steam road wagons for the reward of $10,000 offered by the Wisconsin Legislature, but only two appeared at the appointed time, one from Oshkosh and one from Green Bay. The Green Bay wagon broke down, and after several attempts gave up the contest, being disabled by imperfections in the machinery. The Oshkosh wagon made the trip from Green Bay, by way of Beloit, to Madison, 201 miles, at an average speed of 6 miles an hour, drawing a heavy load over all the grades met. In their report of the test to the Governor, however, the Commissioners, Messrs. Marshall, Olin and Smith, after giving a history of the trip, came to the following conclusions: "The wagon has hauled loads, ploughed and otherwise accomplished in a successful manner every test mentioned in the law or suggested by the commission. They are not, however, satisfied that this machine is, in the language and spirit of the law, a cheap and practical substitute for the use of horses and other animals on the highways and farms. As at present constructed, it is unquestionably of great advantage in ploughing, thrashing, and hauling heavy machines from one farm to another; for heavy teaming on the highway, etc., but the commission is not prepared to say that a machine requiring an outlay of $1,000, with a daily expenditure of from $2 to $6, is a cheap or practical substitute for farm animals. They recommend that the next Legislature make a suitable award for payment in perfecting a machine, but, under all circumstances, cannot make the award of $10,000 offered by the Legislature of two years ago." The Oshkosh wagon, which made the trip successfully, weighs 4,800 lbs. light, or 6,600 lbs. with water and coal for an eight mile run; it hauled a wagon weighing 3,500 lbs. loaded. Its running time for the 201 miles was 33 hours, and at one point it made 21 miles in 2 hours and 10 minutes. Its speed was tried on the Oshkosh race-course, where it made a mile in 4 minutes, 36 seconds. It was also tried at Fort Atkinson with gang ploughs and did excellent work.

IN his twelfth annual report, and referring to the invariability of standards of bronze or gun metal, Mr. Farrar, the Warden of the Standards, notes that the material representative of the Imperial Standard yard is a bar of gun-metal known as Baily's metal, the composition of which (copper 32, tin 5, zinc 2), was selected by the Standards Committee of 1841, and that subsequent experience has shown that this metal is well adapted for its particular purpose. Some recent researches, however, appear to show progressive molecular disturbances in certain gun-metal compositions which might tend to affect the length of any bars constructed of such alloys. With the view, therefore, of determining whether any change has taken place in the lengths of some of the bars constructed by the committee of 1843, the department has been glad to avail itself of the opportunity of forwarding to the Ordnance Survey Office at Southampton some standard measures of brass and bronze, as their comparisons there will tend to throw light on the question of the invariability of measures made of such alloys. There has also been forwarded from Washington the United States bronze Standard No. 11, constructed under the direction of the Standards Committee, 1841, that it may be recompared with the Imperial Standard. Important experiments having been made under the directions of the Admiralty on the tenacity of gun-metal compositions at high temperatures, the Department has been able to submit for experiment some bars of Baily's metal with the view of ascertaining what loss of strength and ductility takes place in this metal at the temperature of boiling water. The expansion of Baily's metal has been shown by M. Fizeau to be from 0 deg. to t deg. $C=0000017972-00000000137\left(40 \text{ deg} - \frac{t}{2}\right)$. Its cubic dilatation will therefore be :—$A=000005227$. $B=000000002662$.

AT the June meeting of the Society for Encouraging National Industry, of France, M. Hélouis exhibited samples of metallic threads and ribbons, "irisated" by means of binoxide of lead, and also samples of lace work ornamented with them. Nobili was the first to obtain such deposits as these on different metals, by electro-chemical means. He immersed a metallic plate, placed in communication with the positive pole of a battery, in a solution of acetate of lead, for example. The negative pole was fastened to a platinum wire, surrounded, except at the ends, by a glass tube; this tube dipping into the liquid in such a way that the free metallic end was placed at a distance of from one to two millimeters from the plate, the

current was passed through it. It was observed that around the wire there were formed concentric rings, produced by delicate films of binoxide of lead, and characterized by varied and extremely brilliant colors, like those exhibited by soap bubbles. Becquerel made an exhaustive study of this phenomenon in 1843. By substituting for acetate of lead a solution of oxide of lead in potassa, or soda, he obtained iridescences that were much more solid, and by taking a certain number of wires as negative poles he was enabled to give objects of small dimensions uniform colorations of such tints as he wished. For certain kinds of objects his process is still in use at the present day. But "irisation" has never before been attempted on ribbons or wires of such delicacy as to measure on an average 32,800 ft. in length to the pound. M. Hélouis has succeeded in giving these delicate threads and bands uniform tints throughout their whole length, and in producing at will any color that he desires.

SPEAKING of the vast—and to the great extent avoidable—destruction of property by fire in the United States, the *Fireman* says that fires are increasing, both in numbers and destructiveness, far more rapidly than the increase of wealth and production. It is computed that from an annual loss by fire in 1868 of $35,000,000, the annual loss, exclusive of exceptional fires such as Boston and Chicago—if they may be called "exceptional"—has increased to $100,000,000. The full significance of this statement cannot be realized unless analyzed. This loss is the irremediable loss of human product and industry. It is the conversion of human blood, brawn and muscle, necessary to create £20,000,000 of value into ashe and smoke. Assuming the labor that produced this value to be worth $3 per day, this loss is the loss of more than the combined labor of 100,000 men for one entire year. Then, too, it must be remembered that this is surplus production. It has been accumulated by producers after earning livelihoods for themselves and families, and paying their share of the cost of government and their proportion of the burdens of society. It would require, then, the labor of 100,000 men for twenty years to replace by surplus production this annual loss. It is not only so much wealth subtracted from the resources of the country, but it is the loss of the productive power of so much capital.

DIVISIBILITY OF THE ELECTRIC LIGHT.—We understand that experiments on a large scale will shortly be made in London and some of the leading provincial towns, of the electric light in its application to the illumination of the streets. Some experiments have already been made, we believe, in June of last year, at the West India Docks, under the superintendence of Mr. Jablochkoff, the inventor of the electric candle, and Mr. Denayrouze, who introduced the new light in Paris. Professor Liebig, Mr. Penn, Professor Abel, from the Royal Laboratory at Woolwich, Mr. Adams, engineer of the Great Eastern, Dr. Ruboch, Major de Winton, and several scientific gentlemen were present at these experiments at Limehouse, and the results arrived at were considered by them to be highly satisfactory. Since that time Mr. P. Jablochkoff has introduced some considerable improvements in the system, which, as is well known, is the property of a company in Paris, formed for the working of the Jablochkoff apparatus. The Place and the Avenue de l'Opera are lighted by the Jablochkoff electric candles, which we have admired in the Pavilion of Electricity at the Paris Exhibition. It remains now to introduce this system in London, Manchester, Birmingham, Liverpool, etc.; and we open to-day a campaign in favor of the adoption of the Jablochkoff light in preference to all other systems. Delegates from several provincial corporations have been sent to Paris in order to report on the advantages of the electric light, and the question has also been discussed in some meetings of gas companies. But these gentlemen did not go to the bottom of the question, and have had only some superficial notions of the results obtained by the inventor; and it is LOT generally known what are the actual power, qualities, and advantages possessed by the Jablochkoff electric candle. The ordinary questions addressed to M. Jablochkoff are the following: (1) How many electric lights can be supplied by a Jablochkoff machine? (2) Given that with that machine a certain number of lights is produced, how many lights will be produced by another machine? (3) Is t possible to have all the lights equal in power and luminosity? (4) Is there any limit to the divisibility of the electric light? To these questions M. Paul Jablochkoff answers as follows, in the most categorical manner, that with any electric machine whatever, by means of his new apparatus, the "amplifier," he not only distributes through a single conductor to several points the current of an unic source of electricity, but also increases it. Therefore, a light can be obtained which may be divided and distributed as easily as water and gas in pipes. M. P Jablochkoff has thus solved the problem of the divisibility of the electric light. Further, from a single source of electricity, he produces at will lights of various power and strength, from the vacillating flame of a night-light to the most brilliant sunlight. Besides, M. Jablochkoff can diminish or increase the light, so that he reduces the most powerful light to the slightest luminous point, *i. e.*, to "the blue," as we say of the gas. All these new apparatus are at work in the Pavilion of the Société-Générale d'Electricité—Jablochkoff patent—opposite the Ecole Militaire at the Champ de Mars. At present, in the works of the Société-Générale d'Electricité, apparatus is being constructed for the complete introduction of electric light instead of gas in the theatre of the Chatelet for the lighting of the express trains on the Kursk Kieff Railway, as well as on the Kharkoff Railway. Another application of the Jablochkoff electric light will, no doubt, attract the attention of the Lords of the Admiralty. The Russian naval authorities have ordered from the Jablochkoff Company some apparatus for the lighting of two men-of-war, containing sufficient power to supply the officers' cabins and all the other lights of the ship; but, when required, these lights can be concentrated in a most intense form, and thrown to an enormous distance when it becomes necessary to look out for torpedo-boats, sunken reefs, or any other of the thousand and one dangers that exist for sailors.